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Clinical Researches on the Action of Diuretic Remedies. By AUSTIN FLINT, M.D., Professor of Clinical Medicine and Medical Pathology in the New Orleans School of Medicine, and of Pathology and Practical Medicine in the Long Island College Hospital.

Various questions having direct bearings on medical practice, arise in connection with the employment of diuretics in different diseases. The uncertainty, as regards the diuretic effect, which belongs to this class of remedies, is well known. To some extent this is intelligible. We can understand why it is difficult to increase the secretion from the kidneys, when these organs are diseased; when, from obstruction of the portal vein in cirrhosis, medicines introduced into the stomach do not readily find their way into the general circulation; when, from obstructive cardiac lesions, the kidneys fail to receive a proper supply of arterial blood, and when the remedies used excite a cathartic action, or when there exists already an afflux of liquids towards the intestinal canal, causing diarrhœa. There are doubtless other circumstances, on the one hand favoring, and on the other hand interfering with, the operation of diuretics, which are not understood.

The *modus operandi* of diuretics must be considered as affording

room for various questions. Some of the remedies of this class are supposed to act by exciting directly the kidneys, when brought to these organs in the arterial blood; others are supposed to act indirectly, by affecting the circulation; and in other modes, it is highly probable that in certain cases the diuretic effect of remedies is due to changes in the blood, or of nutrition, by means of which excrementitious principles eliminated by the kidneys are produced in abundance. Acting in this indirect manner, remedies which are not embraced in the class of diuretics, in some instances, may have a more powerful diuretic effect than the most active remedies of that class.

Other questions relate to the kind of diuretic effect produced. To say that the quantity of urine is increased to a greater or less extent, does not express all that it is desirable to know respecting the operation of diuretics. How is the character of the urine affected, aside from the mere increase in quantity, is an important point of inquiry. A distinction has of late been made between diuretics which increase the quantity of urine mainly by augmenting the proportion of water, and those which increase at the same time the amount of solid constituents of the urine. The former have been called hydrogogue diuretics, and the latter depurative diuretics. This distinction, if just, is undoubtedly a very important one, for it is clear that to fulfill certain indications, as in the treatment of dropsy, the object is to eliminate water; while if it be aimed to expel through the kidneys morbid matter in the blood, the object is to increase the secretion of solids. The justness of this distinction appears to be sustained by the researches of Dr. Hammond on the operation of different diuretics when taken in health.*

The author of a late able work, however, expresses the belief that this distinction does not hold good in disease.† Here, then, is a question of importance. But we may push our inquiries further, and ask whether different diuretics may not increase the different principles in the urine, exclusive of the others. May not some increase the amount of urea, others the urates, others the phosphates, etc.? The importance of this question can be better appreciated when we have obtained facts which enable us to answer it.

The relative efficiency of different diuretics, the various circumstances to be considered in their selection in individual cases, the propriety of prescribing these remedies in cases of renal disease, etc., etc.,

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* *Vide* Am. Journ. Med. Sciences, Jan., 1859.

† Garrod on Gout and Rheumatic Gout. London, 1859.

are other questions pertaining to this subject, which must often suggest themselves to the mind of the medical practitioner.

With our present knowledge of physiology and pathology, we must look to clinical researches for the data by means of which we may hope to arrive at the answers to all these questions. The clinical study of the urine, properly directed and sufficiently extended, is adequate to this end. Something has been already attained, but vastly more remains to be accomplished. I shall offer in this article a slight contribution to this study. The points to which my researches were directed were simply to observe the quantity of urine and the amount of solid constituents under the use of certain diuretics in different cases of disease. The number of cases is only ten. Of these ten cases, in four the disease was *ascites* dependent on cirrhosis; in three, *albuminuria* dependent on *Bright's disease;* and in three, *subacute rheumatism.* These cases came under observation in the Louisville Marine Hospital, in 1852–3, and in 1855–6. Want of leisure has hitherto prevented me from tabulating the results of the examinations of the urine, and ascertaining the conclusions to which they lead.

In making the examinations, the urine passed during the twenty-four hours was preserved, and the specific gravity taken by the urinometer. The amount of solid ingredients was then calculated from the specific gravity and the number of fluid ounces, after the table given by Dr. Bird in his work on urinary deposits.* This method is not strictly accurate, but the results approximate to exactness sufficiently for the objects which I had in view. The difference of a few grains would not affect materially these objects, and, hence, in multiplying the number of fluid ounces contained in the quantity of urine passed in the twenty-four hours, by the amount of solids in a single ounce as determined by the specific gravity, I disregarded the fractions, because the additional labor which would be involved by including these would not be compensated for by the closer approximation to exactness. Had the object been to ascertain the quantity of each or any of the different principles which make up the solids of the urine, more elaborate and difficult processes of examination would have been required, and stricter accuracy important. As it was, the examinations made with reference merely to the quantity of urine and the amount of solids in these ten cases, required considerable pains and perseverance. On this account, I am reluctant to have what

* Page 77, American edition of 1859.

little value they may possess lost; but I wish to say that I attach more importance to them as indicating a direction in which clinical researches may be prosecuted with great advantage to practical medicine, than as embracing in themselves sufficient data for positive conclusions.

In presenting these researches, I shall give first a very condensed abstract of the history of each case; next, in a tabular form, the results of each examination of the urine as regards the date, the quantity of urine in the twenty-four hours, the specific gravity, and the amount of solids, in connection with the treatment; afterwards, appended to each case, a few remarks. Finally, under the head of *General Remarks*, I shall briefly review the facts which have been presented, and give the conclusions which are to be drawn from them.

CASE I. *Ascites—Fatal.*—Michael Davis, aged 30, laborer, Irishman, admitted into the Louisville Marine Hospital Oct. 21, 1852.

Enlargement of the abdomen occurred first, about nine weeks before his admission. Prior to this, for three months he had had much of the time intermittent fever. The abdominal enlargement disappeared in six weeks, and did not return till about two weeks before his admission. In the mean time, he was able to work, until up to about two weeks before his admission. The abdomen measured 41½ inches in circumference. The lower limbs were œdematous. Death occurred in January, 1854. On examination after death, the liver was found contracted and nodulated; the heart, lungs, spleen, and kidneys were healthy. The urine was at the time albuminous. Tapping was performed shortly before death.

October 26th.—The patient had been treated since his admission with hot-air baths once daily, continued for half an hour. They were attended by copious perspiration.

The whole quantity of urine passed in twenty-four hours was 12 oz.; sp. gr., 1030; the whole amount of solids being a fraction over 372 grs. The following table exhibits the treatment pursued during the months of October, November, and December, the quantity of urine passed in the twenty-four hours on the days when examinations were made, together with the amount of solids contained in the urine, and the circumference of the abdomen:

TREATMENT.	DATE.		Quantity of Urine in 24 hrs.	Specific Gravity.	Amount of Solids.	Size of Abdomen.
From Oct. 28th, the following liniment was rubbed daily over the abdomen and thighs: of the tinct. of digitalis, squills and iodine one part, and of the comp. soap liniment two parts.	Oct.	28	14 oz.	1026	378 grs.	37½ inches.
	"	30	25 "	1027	700 "	37½ "
	"	31	24 "	1026	661½ "	37 "
	.....		.....	....	.....	.....
	Nov.	2	36 "	1024	900 "	38 "
	"	3	23 "	1026	621 "	
	"	4 failed to get med.	32 "	1024	800 "	
Nov. 1. The external use of diuretics was discontinued, and the nitrate of potassa, half an ounce in a pint of water, directed daily. The patient failed to get medicine Nov. 4, 5, and 6; he got a double quantity on the 7th, and the quantity prescribed on the 8th, and subsequently till the treatment was changed, Nov. 19th.	"	5 " "	15 "	1028	435 "	36¾ "
	"	6 " "	18 "	1022	396 "	36¾ "
	"	7 got double quantity of medicine.	9 "	1027	252 "	36½ "
	"	8	30 "	1024	750 "	36½ "
	"	9	27 "	1024	675 "	36½ "
	"	11	56 "	1024	1400 "	35¾ "
	"	13	36 "	1028	1044 "	
	"	14	24 "	1025	424 "	36 "
	"	15	35 "	1024	885 "	36 "
	"	16	41 "	1024	1025 "	36 "
	"	17	44 "	1021	924 "	36 "
	.....		.....	....	.....	.....
Nov. 19. Free purgation was produced by elaterium, and the nitrate of potassa suspended.	Nov.	19	9 "	1030	279 "	34½ "
	"	20	11 "	1028	464 "	33 "
	"	21	11 "	1026	299 "	34½ "
	"	22	16 "	1024	400 "	33½ "
Nov. 21 & 22. Moderate purgation was produced by elaterium.	"	23	16 "	1033	560 "	33½ "
	.....		.....	....	.....	.....
	Nov.	24	34 "	1025	624 "	
Nov. 23. The citrate of iron and quinia were prescribed, and continued to the time when the examinations were discontinued, Dec. 9th.	"	25	19 "	1026	516 "	33½ "
	"	26	23 "	1027	644 "	34½ "
	"	27	17½ "	1024	420 "	34 "
	"	28	28 "	1020	560 "	
	"	29	27 "	1024	648 "	33½ "
	"	30	30 "	1020	600 "	34½ "
	Dec.	1	21½ "	1022	472½ "	34 "
	"	2	24 "	1026	648 "	
Dec. 27. The patient was discharged, and he attempted to work; but was obliged to return in three days. Examinations of the urine were not resumed.	"	3	25 "	1023	575 "	
	"	5	25½ "	1021	537½ "	35½ "
	"	6	17½ "	1026	472½ "	34½ "
	"	7	36 "	1028	1044 "	36 "
	"	8	25 "	1026	675 "	35 "
	"	9	28½ "	1028	827½ "	35 "

Remarks.—The examinations of the urine in the foregoing case were made on thirty-eight days. The minimum quantity of urine passed in the twenty-four hours was 9 ounces. This occurred on two occasions. One of these was on a day (Nov. 7—*vide* Table) when an ounce of the nitrate of potassa was taken; but for the three days previous the patient had taken no diuretic medicine, and the diuretic effect of the medicine taken on that day was not manifested until the day following, when the quantity of urine and solid constituents was trebled. The other occasion was when active purgation was pro-

NOTE.—The dotted lines between the figures in each table denote a change in the treatment, as mentioned under the head of *Treatment*.

duced by elaterium. The next lowest quantity of urine was 11 oz. This occurred on two consecutive days, when the patient was purged with elaterium.

The amount of solid constituents in the urine varied on these two occasions, when the quantity of urine was the same. On the first occasion it was 464 grs.. and on the second 299 grs.

The next lowest quantity of urine was 12 oz., the amount of solids being 372 grs. This was when the patient was taking the hot-air baths, which produced copious perspiration.

The external use of the diuretics appears to have induced a decided diuretic effect; the quantity of urine being increased from 12 oz. to 14, 25, and 24 oz., (*vide* Oct. 28, 30, and 31,) and the amount of solids being also increased. That this effect, however, is only apparent, not real, is rendered probable by the fact that the quantity of urine and the amount of solid constituents were habitually as large after Nov. 23d, when all diuretic remedies were suspended. The appearance of a diuretic effect may be due to the fact that the secretory action of the kidneys, prior to the external use of the diuretics, had been diminished by the hot-air baths.

The nitrate of potassa, which was continued, excepting for three days, from Nov. 2d to 19th, manifestly produced a diuretic effect. The quantity given, with the exception of one day, was half an ounce, dissolved in a pint of water, during the twenty-four hours. The quantity of urine for the eleven days that this remedy was taken was greater than after Nov. 24th, when no diuretic remedies were prescribed. The difference, however, is not extremely great. The mean quantity for ten of these days (excluding the day on which a double dose was taken) is $35\frac{1}{5}$ oz. The mean quantity for ten days from Nov. 24, when the patient was taking the citrate of iron and quinia, is $24\frac{9}{10}$ oz. That a diuretic effect was produced, is strikingly shown by the examinations on four consecutive days, during three of which the remedy was suspended, (Nov. 4th, 5th, 6th.) On the first of these days the quantity was not diminished, probably in consequence of the effect of the remedy on the previous day not being exhausted. But on the second, third, and fourth days there was a considerable reduction. And on the day after the remedy was resumed the quantity was notably increased.

A comparison of the amount of solids with the quantity of urine, without going into mathematical calculations, shows a general correspondence between the two. When the quantity of urine was increased from the effect of the nitrate of potassa, the solid constituents

were augmented in about the same proportion. Taking the mean amount for the ten days when the diuretic effect of the remedy just named was manifest, and comparing with the mean amount during the ten days after the patient was placed on the citrate of iron and quinia, we have for the first of these periods a fraction over 864 grs.; and for the second, a fraction over 570 grs. The nitrate of potassa, therefore, as shown by this comparison, increased the amount of the solid constituents of the urine in not far from the proportion in which it increased the quantity of urine.

The effect of hydrogogue cathartics on the urine is shown in the foregoing table. Elaterium was given Nov. 19th, 20th, 21st, 22d. The quantity of urine fell from 44 oz. (which it was on the 17th) to 9 oz.; and for the two following days it was 11 oz. The mean quantity for these four days was $11\frac{3}{4}$ oz., and the mean amount of solid constituents $360\frac{1}{2}$ grs. Directly the elaterium was discontinued, the quantity of urine rose from 16 oz. to 34 oz.

The fluctuations from day to day in the quantity of urine and the amount of solid constituents were probably not greater than those which occur in health; and these, it will be observed, occurred during the time when the diuretic was given, as well as before and after this time.

As regards the effect of the diuretic on the size of the abdomen, it will be perceived that it was negative. The circumference of the abdomen, in fact, was less during the time that the patient was taking the citrate of iron and quinia than during the time that the nitrate of potassa was given. It was distinctly, but not greatly, lessened during the days when purging with elaterium was resorted to. The object of the treatment with the diuretic was to relieve the ascites. Not only was this object not effected, but it is probable that the increased amount of solids in the urine, caused by the diuretic, was a source of injury to the patient. The history of the case does not show evidence of improvement in any respect while the diuretic was given; but subsequently, when the patient had taken the citrate of iron and quinia for a month, the general improvement was such that the patient left the hospital and attempted to go to work. In short, this case affords no encouragement for the use of the nitrate of potassa in the treatment of ascites dependent on cirrhosis.

Case 2. *Albuminuria, (Bright's Disease)—Fatal.*—John Mooney, aged 35, Irishman, laborer, was admitted into the Louisville Marine Hospital October 24, 1854. He had anasarca, and the urine was highly albuminous. He also had intermittent fever, of the quotidian

type. The latter was arrested by the sulphate of quinia. October 28th, the air-bath was employed at night, causing profuse perspiration. November 1st, the air-bath was discontinued, and he had no medicinal treatment up to November 19th, when the iodide of potassium was prescribed, in doses of a fraction over three grains three times daily. This remedy was continued up to the day of his death, November 30. Vomiting and purging were prominent symptoms, and it is probable that the remedy was not always retained. The dying was by asthenia, and the kidneys were found after death to be affected with Bright's disease.

The following table exhibits the results of examinations of the urine on sixteen days during the last month of the patient's life:

Treatment.	Date.	Quantity of Urine in 24 hrs.	Specific Gravity.	Solid constituents in 24 hours.
The treatment from	Oct. 28	22 oz.	1015	330 grs.
October 28th to Nov.	" 29	22 "	1012	264 "
1st was the hot-air	.....	.....	....	.....
bath. The patient was	Nov. 2	18 "	1008	134 "
also taking the sulphate	" 6	51 "	1012	512 "
of quinia.	" 8	36 "	1011	396 "
	" 9	23 "	1012	276 "
	" 10	25 "	1013	325 "
	" 11	30 "	1012	360 "
Nov. 19, the iodide	" 14	12 "	1014	252 "
of potassium was pre-	" 17	25 "	1010	250 "
scribed, in doses of a	.....	.....	....	.....
fraction over three	" 19	20 "	1011	220 "
grains three times a	" 20	19 "	1028	551 "
day; this was continued	" 21	14 "	1013	182 "
until death.	" 22	23 "	1010	230 "
Death occurred Nov.	" 24	15 "	1014	210 "
30th.	" 25	6 "	1025	156 "

Remarks.—The examinations in this case, continued during the last month of life, show, with occasional fluctuations, a steady decline in the quantity of urine and the amount of solids. The low specific gravity, or the small proportion of solids to the quantity of the urine, is characteristic of the disease. The iodide of potassium produced no apparent diuretic effect. The mean quantity of urine from Nov. 2d to 17th inclusive—the patient during this time taking no medicine—is $23\frac{6}{8}$ oz.; from Nov. 19th to 25th inclusive, when the iodide of potassium was given, is $16\frac{1}{2}$ oz. The mean amount of solids for the first of these periods is $313\frac{1}{3}$ grs.; for the second period, $258\frac{1}{3}$ grs. The difference in the amount of solids between the two periods, thus, is not far from corresponding with the difference in the quantity of urine.

It is to be considered that the quantity of the iodide of potassium given daily was not large; and also, that, owing to frequent vomiting, it may frequently have been rejected. It is gratuitous to add, that the negative effect of this remedy in this case is not to be taken as any criterion of its operation in other forms of disease, or when the kidneys are not the seat of degenerative disease.

Case 3. *Subacute Rheumatism.*—Philip Hesse, aged 29, German, laborer, was admitted Nov. 11, 1853. He had been affected with subacute articular rheumatism for fourteen days. The ankles, toes, wrists, and finger-joints had been attacked. The wrists, ankles, and shoulders were tender and painful at the time of his admission. The ankles and wrists were swelled. There was no febrile movement. He was subject to intermittent fever. He progressively improved, and was discharged quite well Dec. 5, 1853. The treatment consisted exclusively of the administration of the acetate of potassa. The patient took of this remedy two drachms daily, in a pint of mucilaginous liquid, from Nov. 12 till Nov. 15, when the quantity of potash was increased to four drachms. Nov. 21st the potash was diminished to two drachms, and on the 26th it was discontinued. The following table shows the results of examinations of the urine from Nov. 12 to Dec. 2:

Treatment.	Date.	Quantity of Urine in 24 hrs.	Specific Gravity.	Amount of Solids in 24 hours.
Acetate of potassa	Nov. 12	12 oz.	1032	396 grs.
3ij. in Oj. of mucilaginous liquid was prescribed Nov. 12. The first examination on the date just stated was prior to entering on the treatment.	" 14	30 "	1020	600 "
	" 15	29 "	1020	580 "
	" 16	30 "	1018	540 "
	" 18	40 "	1018	720 "
	" 19	34 "	1022	748 "
	" 20	29 "	1021	509 "
	" 21	25 "	1022	550 "
Nov. 15, the potash was increased to 3iv. in the 24 hours.	" 23	29 "	1025	754 "
	" 24	29 "	1024	696 "
	" 25	17 "	1024	408 "
Nov. 21, the potash was diminished to 3ij. in the 24 hours.	" 26	30 "	1018	540 "
	" 27	29 "	1015	434 "
	" 28	29 "	1027	812 "
Nov. 26, the potash was discontinued, and no medicine afterwards.	" 29	36 "	1025	936 "
	" 30	29 "	1028	848 "
	Dec. 1	32 "	1024	768 "
Dec. 5, discharged, quite well.	" 2	29 "	1017	493 "

Remarks.—Taking the first examination in the foregoing case (made before the acetate of potash was prescribed) as a criterion of

the quantity of urine and the amount of solids prior to the administration of the diuretic remedy, the effect of the remedy was apparently immediate and considerable. The effect of an increased quantity of the remedy (November 15) appears to be also marked in the increased quantity of urine and amount of solids. November 18th and 19th, this effect seems to be farther shown by the lessened amount of solids; November 25th, 26th, and 27th, the acetate of potassa having been diminished from four to two drachms. But it is to be remarked, that after Nov. 26th, when the remedy was discontinued, the quantity of urine and the amount of solids were greater than before. The maximum amount of solids occurred during the time that no medicine was given; and the mean quantity of urine and amount of solids for the seven days on which examinations were made after the medicine was discontinued, are somewhat greater than for the preceding seven days. The amount of solids after the medicine was discontinued, it is to be noted, considerably exceeded the average amount of health, assuming the latter to be about 650 grs,; while the quantity of urine did not exceed the average quantity in health, viz., from 30 to 40 oz.

Case 4. *Subacute Rheumatism.*—John Adams, aged 36, Mexican, laborer, admitted January 7, 1854. He had had repeatedly attacks of rheumatism. Fifteen days before his admission, he was attacked with subacute articular rheumatism, affecting, successively, various joints. When admitted, and afterwards, he was able to be up and about the ward. Several joints were tender and painful, without redness or swelling. He had mitral valvular lesions and enlargement of the heart. The acetate of potash was the only remedy prescribed. January 15th, he began with this remedy, and took four drachms in two pints of water during the twenty-four hours. January 16th, the quantity was reduced to two drachms in the twenty-four hours. This quantity was continued until his discharge, Jan. 24th, when he was free from rheumatic trouble, and complained only of dyspnœa on exercise, referable to the cardiac affection.

The following table shows the results of examinations of the urine Jan. 14, before commencing to take the potash, and subsequently, up to the date of his discharge:

Treatment.	Date.	Quantity of Urine in 24 hrs.	Specific Gravity.	Amount of Solids in 24 hours.
	Jan. 14	31 oz.	1027	868 grs.
Jan. 15. He took	.....	.....	....	.....
four drachms of the	" 16	60 "	1013	780 "
acetate of potassa; af-	" 17	65 "	1016	1040 "
terwards he took two	" 18	63½ "	Not taken.	

drachms daily in a pint of water, up to the day of his discharge. The improvement was progressive.	Jan. 19	72½ oz.	1013	943 grs.
	" 20	67 "	1013	871 "
	" 21	65 "	1014	910 "
	" 22	74 "	Not taken.	"
	" 23	76 "	1014	1067 "
	" 24	82 "	1014	1148 "

Remarks.—In this case, considering the quantity of urine and the amount of solids prior to commencing to give the acetate of potassa, as represented by the result of the examination Jan. 14th, an immediate, marked diuretic effect is apparent. The diuretic effect is farther shown by the quantity of urine and the amount of solids being greatly above the averages of health. The amount of solids is much larger than in health, but it will be seen that the quantity of urine is increased in a larger proportion. It would have been interesting in this case to have kept the patient under observation for some time after suspending the medicine, and continuing the daily examinations of the urine.

Case V. *Ascites.*—Michael Collins, aged 22, Irish, laborer, was in hospital when my service commenced, in October, 1855, having been admitted in the preceding May. When he entered, the abdomen was greatly enlarged with ascites. The dropsy was now much less, and œdema of the limbs (which had also existed) had disappeared. He had improved during his stay in the hospital, in all respects. The urine was not albuminous.

Oct. 3d.—The quantity of urine passed during the twenty-four hours was ascertained, and its specific gravity; the patient at this time taking no medicine. The nitrate of potassa was then prescribed, a drachm, largely diluted, to be given during the day. This treatment was continued to October 14th, and examinations of the urine made. On this date, the citrate of iron and quinia were prescribed.

Oct. 22.—He reported well enough to go to work, and he was discharged.

The following table shows the results of the examinations of the urine from the 3d to the 14th of October:

Treatment.	Date.	Quantity of Urine in the 24 hours.	Specific Gravity.	Amount of Solids in the 24 hours.
Oct. 3, no treatment.	Oct. 3	30 oz.	1024	720 grs.
Oct. 4, the nitrate of potash, 3j., largely diluted, in the 24 hours. This con-	Oct. 6*	42 oz.	1022	924 grs.
	" 7	36 "	1024	864 "
	" 9	30 "	1024	720 "
	" 10	36 "	1024	864 "

* Abdomen measured 33 inches in circumference.

tinued till Oct. 14.	Oct. 11	30 oz.	1026	810 grs.
	" 12	31 "	1025	806 "
	" 14*	32 "	1025	832 "

Remarks.—In this case, the quantity of urine was slightly increased, while the patient was taking a drachm, daily, of the nitrate of potassa, but the quantity, one day only, (Oct. 6,) exceeded the limit of health. The amount of solids exceeded the healthy average before the remedy was given, but during the use of the remedy these were considerably increased. In case No. 4, the quantity of urine was increased out of proportion to the increase in the amount of solids, but in this case the effect was the reverse; the amount of solids was increased out of proportion to the quantity of urine, the latter (with the exception of a single day) being within the average quantity in health.

In this case, it is to be regretted that the examinations of the urine were not continued after the diuretic remedy was suspended.

CASE VI. *Subacute Rheumatism.*—Patrick Fogerty, aged 24, laborer, Irishman, was admitted October 15, 1855. He had had subacute rheumatism for four weeks. The two weeks preceding his admission he had kept the bed. He was now able to get up. The shoulder and ankle-joints had been chiefly affected. He had previously had the disease. There was no affection of the heart; he was subject to intermittent fever.

Oct. 17.—The urine was preserved and the specific gravity ascertained. Up to this time no treatment was pursued. On the 17th a drachm of the acetate of potassa, daily, largely diluted, was prescribed. This treatment was continued to the 23d of October. The acetate of potassa was then suspended, and twenty grains of the sulphate of quinia, daily, prescribed, with Dover's powder, *pro re nata.* Up to this date, there had been no improvement. The sulphate of quinia was given till Nov. 2d. The rheumatic affection continued, but was diminished. Nov. 2d, a fluid drachm of the wine of colchicum, three times daily, was prescribed. The colchicum acted freely on the bowels, without producing vomiting. The rheumatic affection was relieved. Nov. 6th, the colchicum was suspended, and half a grain of calomel, every two hours, prescribed. This was continued until slight ptyalism was induced. Dec. 9th, the patient was nearly free from the rheumatic affection. The date of discharge is not noted.

The following table shows the results of examinations before any treatment was adopted, and subsequently, while the patient was taking successively the acetate of potassa, the sulphate of quinia, and the

* Abdomen measured 30 inches in circumference.

wine of colchicum. The examinations were not continued after the colchicum was suspended, and mercurialization was resorted to.

Treatment.	Date.	Quantity of Urine in the 24 hours.	Specific Gravity.	Amount of Solids in the 24 hours.
Oct. 17, examination prior to treatment.	Oct. 17	17 oz.	1030	527 grs.
.....	.....	.....	....	.....
On this day a drachm of the acetate of potassa daily.	Oct. 18	22 oz.	1030	682 grs.
	" 19	41 "	1015	615 "
	" 20	26 "	1020	520 "
	" 21	30 "	1025	806 "
Oct. 23, the acetate of potassa suspended, and S. quinia, grs. xx., daily, substituted.	" 22	28 "	1020	760 "
.....	.....	.....	....	.....
	Oct. 24	35 oz.	1025	910 grs.
	" 25	41 "	1030	1271 "
	" 26	32 "	1025	832 "
Nov. 1, the quinia discontinued, and wine of colchicum, 5j. three times, substituted; the latter acted freely as a purgative. It was discontinued on Nov. 6, and mercurialization resorted to; but the examinations were not continued.	" 27	46 "	1025	936 "
	" 28	40 "	1020	840 "
	" 29	42 "	1020	882 "
	" 30	40 "	1020	840 "
	" 31	40 "	1025	1000 "
	Nov. 1	32 "	1025	832 "
.....	.....	.....	....	.....
	Nov. 2	33 oz.	1030	1023 grs.
	" 3	34 "	1030	1054 "
	" 4	40 "	1025	1040 "
	" 5	41 "	1025	1066 "

Remarks.—On commencing the acetate of potassa in this case, the quantity of urine and the amount of solids were at once increased, but the mean quantity of urine for the five days that this remedy was given was below the lowest average of health, viz., 29⅗ oz. The mean amount of solids for this period was slightly above the average of health, 676⅘ grs.

The table shows the remarkable fact, that during the nine days that the patient was taking twenty grains of the sulphate of quinia, daily, the quantity of urine and the amount of solids were considerably greater than when he was taking the acetate of potassa. From this comparison, it would appear that the quinia in this instance was, of the two remedies, much the more powerful diuretic. Was this merely a coincidence? Comparative examinations in other cases are necessary in order to answer this question.

The examinations for four days while the patient was taking the wine of colchicum, show a still greater increase in the amount of solids, the amount being much above the healthy average.

It is to be remarked in connection with the increased amount of

solids, while the patient was taking the sulphate of quinia and the colchicum, that marked improvement took place. It is also worthy of note, that the colchicum produced free purging, acting apparently, at the same time, as a diuretic.

CASE VII. *Ascites—Fatal.*—Richard Gordon, aged 32, Irishman, laborer, of intemperate habits, admitted Oct. 23d, 1854. Had been subject to frequent attacks of intermittent fever. He first noticed enlargement of the abdomen six weeks before his admission. Œdema of the lower extremities followed; the abdomen had continued to enlarge; the urine was not albuminous; the abdomen was largely distended with liquid; he had taken elaterium and various diuretics prior to his admission.

Oct. 24th, he was purged freely with elaterium.

Oct. 25th, the following liniment was directed to be applied freely, with friction, twice daily, over the abdomen: of the tinctures of squill, digitalis, and iodine, each, one part, and of soap liniment, two parts. The ingestion of liquids to be restricted.

Oct. 26th, purging with elaterium was again resorted to, but the external application of the diuretic mixture was continued to Nov. 1. On this date he was tapped, and the nitrate of potassa, a drachm, daily, was prescribed. Between Nov. 1 and Dec. 8, he was tapped three times. Dec. 18th, he was again tapped, and again Dec. 31st, and Jan. 10th. He left the hospital Jan. 11th, 1856, and died Feb. 2d.

The following table shows the results of examinations while the external diuretics were employed, and subsequently, when the patient was taking the nitrate of potassa:

Treatment.	Date.	Quantity of Urine in the 24 hours.	Specific Gravity.	Amount of Solids in the 24 hours.
Oct. 24, elaterium was prescribed.	Oct. 25	6 oz.	1030	180 grs.
.....	.....	.....	....	.....
Oct. 25, external application of diuretics commenced.	Oct. 26	4 oz.	1032	132 grs.
	" 27	7 "	1032	231 "
Nov. 1, tapped, and nitrate of potassa, a drachm daily, prescribed.	" 28	7 "	1030	217 "
	" 29	7 "	1030	217 "
	" 30	8 "	1032	264 "
.....	.....	.....	....	.....
The abdomen was rapidly filling at the time the examinations were discontinued, and he was subsequently tapped repeatedly.	Nov. 1	16 oz.	1030	481 grs.
	" 2	16 "	1030	481 "
	" 4	18 "	1025	468 "

Remarks.—On the day after the purging with elaterium, (Oct. 25,) the quantity of urine and amount of solids were extremely small, probably having been diminished by the purgation. The quantity of the urine and amount of solids were greater, but still very much be-

low the healthy average, while the diuretic mixture was applied externally. There was a decided increase both in the quantity of urine and amount of solids when the nitrate of potassa was given directly after the first tapping; both, however, still falling considerably below the average of health.

This table illustrates the impeded function of the kidneys in cirrhosis.

Case VIII. *Albuminuria—Bright's Disease—Fatal.*—Michael McCran, of intemperate habits, aged 45, laborer, admitted Nov. 9, 1855. He had had intermittent fever much of the time for the two months preceding his admission. Œdema of the lower extremities occurred soon after being attacked with intermittent fever, and œdema of the face about the same time. When he entered, he was anasarcous, and there was liquid effusion in the chest and abdomen. The urine was highly albuminous. The heart was free from disease. Vomiting and diarrhœa had occurred frequently before his admission.

Nov. 13.—The external application of the diuretic mixture used in previous cases was directed, and the citrate of iron and quinia internally. This treatment was continued till Nov. 21. During this period, diarrhœa was a prominent symptom. The citrate of iron and quinia were given alone from Nov. 21st to 25th; Nov. 25th, a grain of digitalis and a grain of squill were prescribed, in combination, three times daily. Gin was also directed in lieu of porter, which he had previously taken, and an infusion of juniper.

Nov. 30.—The digitalis and squill were suspended, and elaterium was prescribed.

Dec. 1.—A drachm of the nitrate of potassa, three times daily, was prescribed, and continued for three days. It appeared to act as a cathartic, and the general dropsy was somewhat diminished; but the patient was extremely feeble. From Dec. 3d to 7th, no medicine was prescribed.

Dec. 7.—The external application of the diuretic mixture was again commenced, and continued till Dec. 11th. The diarrhœa persisted, and he vomited frequently.

Dec. 11.—The external use of the diuretics was discontinued, and the citrate of iron and quinia prescribed.

Dec. 21.—Gallic acid was substituted for the citrate of iron and quinia. Death occurred December 25th.

The kidneys were enlarged and granulated, probably from fatty degeneration. The liver was cirrhosed and contracted.

The following table shows the results of examinations during the varied treatment just detailed:

Treatment.	Date.	Quantity of Urine in 24 hrs.	Specific Gravity.	Amount of Solids in 24 hours.
Prior to Nov. 13th, no remedy was given.	Nov. 12	20 oz.	1015	300 grs.
	.....	.....	....	.....
Nov. 13, external application of diuretics, and the citrate of iron and quinia internally.	" 13	14 "	1015	210 "
	" 14	20 "	1015	300 "
	" 15	21 "	1020	420 "
	" 16	19 "	1025	694 "
	" 17	26 "	1030	806 "
Nov. 21, external use of diuretics, suspended on account of their causing irritation of the skin; the citrate of iron and quinia continued.	" 19	40 "	1030	1240 "
	" 20	37 "	1030	1147 "
	.....	.....	....	.....
	" 21	22 "	1025	572 "
	" 22	28 "	1020	360 "
	" 23	26 "	1015	390 "
Nov. 26, a grain of digitalis and a grain of squill, three times daily, with gin, and an infusion of juniper.	" 24	28 "	1015	260 "
	" 25	25 "	1015	375 "
	.....	.....	....	.....
	" 26	15 "	1020	300 "
	" 27	19 "	1020	380 "
	" 28	22 "	1020	440 "
Dec. 1, nitrate of potassa, 3j three times daily, for three days.	" 29	16 "	1020	320 "
	" 30	10 "	1020	200 "
	.....	.....	....	.....
	Dec. 1	14 "	1020	280 "
From Dec. 3d to 7th, the citrate of iron and quinia.	" 2	17 "	1020	340 "
	" 3	16 "	1015	240 "
	.....	.....	....	.....
	" 4	15 "	1015	225 "
Dec. 7, the external use of the diuretic mixture resumed.	" 5	14 "	1015	210 "
	" 6	16 "	1015	240 "
	.....	.....	....	.....
	" 7	14 "	1020	280 "
Dec. 11, external use of diuretic mixture suspended, and the citrate of iron and quinia given.	" 8	13 "	1019	247 "
	" 9	19 "	1018	343 "
	" 10	13½ "	1018	243 "
	.....	.....	....	.....
	" 11	10 "	1018	180 "
The examinations were not continued after Dec. 17th.	" 12	8 "	1017	136 "
	" 13	12 "	1016	192 "
	" 14	11 "	1016	176 "
Death occurred, Dec. 25th.	" *16	35 "	1015	525 "
	" 17	20 "	1015	210 "

Remarks.—The urine in this case, prior to entering upon any medicinal treatment, (Nov. 12,) was deficient in quantity and in solid ingredients; a condition belonging to Bright's disease. The external use of diuretics, in this instance, appears to have increased the quan-

* It is noted that the urine was suppressed on the 15th.

tity of urine, and still more, the amount of solids. The latter is to be remarked the more because the diuretics applied (digitalis, squill, and iodine,) are those which have been considered as increasing the quantity disproportionately to the increase of solids. The reverse seems to have been the effect in this instance. That this effect was real, not merely apparent, is rendered probable by the notable diminution in the amount of solids after Nov. 21, when the external use of the diuretics was suspended, and the patient was taking only the citrate of iron and quinia. Digitalis and squill, taken internally, in the dose of a grain of each, three times daily, produced no diuretic effect; the quantity of urine and the amount of solids being less than when the patient was taking the citrate of iron and quinia. The nitrate of potassa, given for three days also, did not operate as a diuretic. This may be owing to the remedy having acted as a cathartic. The external use of the diuretics, which was resumed on the 7th of December, produced little or no diuretic effect. From December 11th to 17th, it will be observed that, with the exception of a single day, (Dec. 16th,) the quantity of urine and amount of solids were extremely small. This was near the close of life, and the patient was taking only the citrate of iron and quinia. The unusual increase in quantity and solids on Dec. 16 is to be explained, perhaps, by the fact that there was no discharge of urine the day previous.

In looking over the table, the uniformity for successive days, repeatedly, as regards the specific gravity, while the quantity of urine varied, is to be remarked. This is the more remarkable when it is considered that the kidneys were greatly degenerated, and when it would have been anticipated that the fluctuations in the specific gravity would be greater than under other circumstances.

Case IX. *Albuminuria—Bright's Disease.* — Edward Kennan, Irishman, intemperate, laborer, aged 25, was admitted Nov. 27, 1855. He had been subject to frequent attacks of intermittent fever for five years. Œdema of the lower extremities had existed for eighteen months. He kept, however, at work until five weeks before his admission. When he entered he was moderately anasarcous; vision was impaired; the urine was albuminous; the heart was free from disease; he was able to be up and about the ward.

Nov. 28.—Digitalis and squill, of each one grain, in combination, three times, daily, were prescribed, and a pint of the infusion of juniper, to be taken during the twenty-four hours.

Dec. 7.—The patient's condition was much improved, except that the indistinctness of vision remained the same. The œdema had

greatly diminished. The digitalis and squill were now discontinued, and half an ounce of the nitrate of potassa, daily, substituted.

Dec. 11.—The œdema was nearly gone, and the general improvement was marked. The vision was the same. The nitrate of potassa was discontinued, and the citrate of iron and quinia prescribed.

The examinations were not continued after this date. The patient subsequently took gallic acid; he continued to improve; the vision became better; the œdema disappeared entirely; the urine, however, continued albuminous; his countenance was pallid.

He was discharged Jan. 25th, 1860. He had lost during his stay in hospital 33 lbs. in weight.

The following table shows the results of examinations up to Dec. 11, 1855:

Treatment.	Date.	Quantity of Urine in 24 hrs.	Specific Gravity.	Amount of Solids in 24 hours.
The examination Nov. 28th was prior to any remedy being given; on this date was prescribed digitalis and squill, with infusion of juniper.	Nov. 28	48 oz.	1010	480 grs.
	.....	.....	....	.....
	" 29	55 "	1010	550 "
	" 30	70 "	1010	700 "
	Dec. 1	56 "	1010	560 "
	" 2	55 "	1010	550 "
	" 3	51 "	1010	510 "
	" 4	50 "	1010	500 "
	" 5	49 "	1010	490 "
	" 6	52 "	1010	520 "
	.....	.....	....	.....
Dec. 7, half an ounce of the nitrate of potassa, daily, continued to the 17th. At this date the dropsy nearly gone, and subsequently no diuretics were given.	" 7	56 "	1010	560 "
	" 8	57 "	1010	570 "
	" 9	57 "	1010	570 "
	" 10	79 "	1012	948 "
	" 11	83 "	1012	996 "
	" 12	86 "	1012	1032 "
	" 13	75 "	1010	750 "
	" 14	77 "	1010	770 "
Patient discharged free from dropsy, Jan. 25th.	" 15	90 "	1010	900 "
	" 16	97 "	1011	1067 "
	" 17	80 "	1010	800 "

Remarks.—The examinations show in this case a marked diuretic effect of the digitalis, squill, and juniper. They show a much more marked effect from the nitrate of potassa, given in the dose of half an ounce, daily. The uniformity in the specific gravity in nearly all the examinations is truly remarkable. This shows an exact proportion in the increase of the solids to the increased quantity of urine. This fact, it will be observed, holds good alike when the diuretic remedy

consisted of digitalis, squill and juniper, and when the nitrate of potassa was prescribed.

So far as the dropsy was concerned, the diuretic treatment in this case was successful. The patient's condition in other respects was also much improved. The case illustrates the efficiency of diuretics in certain cases of albuminuria.

Case X. *Ascites.*—John Battersley, aged 43, Irishman, laborer, admitted Sept. 12, 1855. He presented, when my service commenced, (Oct. 1, 1855,) moderate ascites. No œdema. He was up and about the ward, complaining only of debility.

Oct. 5.—A drachm of the nitrate of potassa, largely diluted, three times daily, was prescribed. This was continued to Oct. 14th. The peritoneal effusion had then nearly disappeared. The nitrate of potassa was discontinued, and the citrate of iron and quinia prescribed. He progressively improved, and was discharged Nov. 11th, 1855, free from ascites.

The following table shows the examinations made while the patient was taking the nitrate of potassa:

Treatment.	Date.	Quantity of Urine in 24 hrs.	Specific Gravity.	Amount of Solids in 24 hours.
The examination Oct. 5 was prior to the remedy being given; on that date a drachm of the nitrate of potassa was given, three times daily; this was continued till Oct. 14th. The patient was discharged free from ascites, Nov. 11th.	Oct. 5	32 oz.	1020	640 grs.
	.....	.....	....	.....
	" 6	32 "	1021	672 "
	" 7	26 "	1024	624 "
	" 9	48 "	1020	960 "
	" 10	46 "	1016	736 "
	" 11	26 "	1020	520 "
	" 12	31 "	1025	806 "

Remarks.—The nitrate of potassa in this case produced apparently a distinct, but not very marked, diuretic effect. The increased amount of solids under the use of the remedy was greater than the increased quantity of urine.

The case illustrates the relief of moderate ascites under the use of a diuretic remedy.

General Remarks.—The series of examinations of the urine in the foregoing ten cases may be reviewed from two points of view: *first*, as regards the operation of the remedies, severally; and *second*, as regards the diseases. Directing attention in the first place to the diuretic remedies, we will consider each under a separate head.

Acetate of Potassa.—This remedy was given in three cases, all of which were cases of subacute rheumatism, viz., cases 3, 4, 5, and 6. In each of these cases, there was an immediate increase in the quantity

of urine and the amount of solids when the patient began to take the remedy. In one of the cases, (case 4,) the increase in quantity exceeded the increase of solids. In the other cases, the quantity and solids were increased about equally. In one case, (case 3,) the quantity and solids decreased when the remedy was given in smaller doses, but subsequently both the quantity and solids became greater when the remedy was discontinued. This shows that there may be a liability to error by imputing an augmentation of the urinary secretion, occurring spontaneously in the course of this disease, to the operation of a diuretic remedy. A series of examinations in a number of cases in which diuretics were not given, would be valuable, as showing to what extent changes occur irrespective of diuretics. In another case, (case 6,) although an immediate diuretic effect from the remedy was apparent, yet both the quantity of urine and the solids became still more increased after the remedy was suspended, and the patient was taking twenty grains of the sulphate of quinia daily. Whether the latter fact be owing to the quinia, or to changes occurring spontaneously in the course of the disease, we cannot say, without researches in other cases to aid in forming an opinion. In this case a drachm only of the acetate of potassa daily was given, and continued for five days, the quantity given in the other cases being two and four drachms daily. The actual amount of solids and quantity of urine in this case were but little above the average of health, while the patient was taking the acetate of potassa. In case 3 the increased quantity and solids did not greatly exceed the limit of healthy averages; but in case 4 the augmentation, both in quantity and solids, was great, the maximum of quantity being eighty-two fluid ounces, and of the amount of solids eleven hundred and forty-eight grains.

In conclusion, these cases afford evidence of the value of the acetate of potassa as a diuretic remedy given especially for the purpose of increasing the solid constituents of the urine.

Nitrate of Potassa.—This remedy was given in six cases, viz., cases 1, 5, 7, 8, 9, 10. In four of these cases the patients were affected with ascites dependent on cirrhosis; in two of the cases, the disease was albuminuria dependent on Bright's disease. In all of these cases, save one, there was an immediate increase of the quantity of urine and the amount of solids when the remedy was given. In the excepted case, (case 8,) the remedy was given on three days only, and it appeared to act as a cathartic. The disease in that case was albuminuria, and vomiting and purging were prominent symptoms. Moreover, the remedy was given but a short time before the fatal termina-

tion of the disease. The quantity of the salt given during the twenty-four hours, in the different cases, varied from one to four drachms. That the diuretic effect was real, as well as apparent, is shown in one case, (case 1,) by the decrease of the urinary secretion when the remedy was discontinued, and again an increase when the remedy was resumed. Of the cases of ascites, in all the diuretic effect was moderate. In case 7, the quantity of urine and the amount of solids, notwithstanding the diuretic effect, fell considerably short of the averages in health. In case 1, both the quantity and solids exceeded the healthy limit; the maximum of the former being fifty-six fluid ounces, and of the latter fourteen hundred grains. The same was true in cases 5 and 10, but to a less extent; the maximum of quantity in case 10 was forty-eight ounces, and of solids nine hundred and sixty grains; and in case 5 the maximum of quantity was forty-two ounces, and of solids nine hundred and twenty-four grains. As regards the relative increase of the quantity of urine and the amount of solids in two of the cases of ascites, the augmentation of each was not far from equal; and in the other two cases, the amount of solids was increased more than the quantity of the urine. In one of the two cases of albuminuria, the diuretic effect was very marked, (case 9.) The quantity and solids were increased equally, the specific gravity remaining with very little variation. The maximum of quantity in this case was ninety fluid ounces; and of the amount of solids, one thousand and sixty-seven grains.

These cases attest the value of the nitrate of potassa, especially as regards the augmentation of the solids in the urine.

Digitalis, Squill and Juniper.—These remedies were given in conjunction in two cases, viz., cases 8 and 9. In both, a grain of digitalis and squill was given three times daily; in one of the cases, a pint of the infusion of juniper was directed to be taken during the day; and in the other case, the quantity is not noted. In case 8, these remedies were continued for five days. No diuretic effect was produced. The quantity of urine and the amount of solids were less than subsequently, when the patient was taking only the citrate of iron and quinia. The same is true of the nitrate of potassa, which was substituted for the vegetable diuretics. The disease in this case was albuminuria, which proved fatal, and vomiting and purging were prominent symptoms.

I case 9, the remedies were continued for eight days. In this case a marked diuretic effect followed. The quantity of urine and the amount of solids were increased in exactly the same degree, showing

that in this instance these remedies did not act as hydragogue diuretics; in other words, increasing merely the elimination of water. The nitrate of potassa in this case, given subsequently, produced a much more marked diuretic effect, the quantity and solids preserving the same relative proportion. In this case the disease was also albuminuria, the patient becoming well enough to leave the hospital.

These cases are, of course, too few to warrant any important conclusions. The equal increase of the quantity of urine and the amount of solids, in case 9, is, however, a fact of importance.

Iodide of Potassium.—This remedy was given in one case only, a fatal case of albuminuria, (case 2.) The dose was a fraction over three grains, three times daily. It was given in the latter part of the disease, and continued to the time of death. Vomiting and purging were prominent symptoms. Examinations of the urine for six days showed no diuretic effect. The circumstances were extremely unfavorable for a diuretic effect from this or any other remedy; and, moreover, the quantity of the remedy given was small.

Wine of Colchicum.—In one of the cases of subacute rheumatism, (case 6,) a drachm of the wine of colchicum was given three times daily, for five days. It produced vomiting the first day, but not afterwards; but moderate purging continued during the time it was given. There was a marked increase in the amount of solids while the remedy was given, but the quantity of urine was not increased. This remedy was preceded by the sulphate of quinia, twenty grains daily, and by the acetate of potassa. The acetate of potassa increased the quantity and the solids of the urine, but both were still more increased while the patient was taking the quinia. The colchicum appeared to render the solids still more abundant, but there is room for the suspicion that this progressive increase may have been due to changes belonging to the course of the disease.

External Use of Diuretics.—In three cases, viz., cases 1, 7 and 8, a diuretic mixture was freely applied over the abdomen, with brisk friction continued for some time, once or twice daily. This was in imitation of a method proposed some years since by Prof. Christison. Prof. C., however, advised the continued application of diuretics to the surface by means of saturated cloths or the spongio piline. The application by means simply of an embrocation is doubtless less efficient, but I have known it to be followed by a marked diuretic effect. The mixture employed in all these cases was composed of the tinctures of digitalis, squill and iodine, of each one part, combined with two parts of soap liniment.

In case 1, a fatal case of ascites, the application was continued for three days, and a comparison with the quantity of urine and the amount of solids before this treatment was commenced, showed an apparent effect; but before commencing the treatment, the hot-air bath had been employed, and this probably had diminished the urinary secretion. The comparatively increased quantity of solids under the external use of the diuretics continued after this treatment was discontinued, and no diuretic given.

In case 7, there was an increase in the quantity and solids, when the diuretics were applied; but in this case, as in case 1, there was ground to suppose that the effect was more apparent than real, because, prior to entering on the treatment, the patient had been purged with elaterium. The treatment was continued for five days. The disease in this case was ascites, which proved fatal. Subsequently the quantity of urine and of solids increased under the internal use of the nitrate of potassa.

In case 8, a fatal case of albuminuria, the treatment was continued for seven days, and at a later stage resumed, and continued for four days. When first employed, the quantity of urine was somewhat increased, and the solids more than the quantity. That this effect was in reality due to the treatment, was shown by a decrease in the quantity and solids, when it was discontinued. When employed a second time, at a late period in the disease, no effect was produced.

On the whole, these few trials suffice to afford encouragement for resorting to the external use of diuretics in cases in which there are difficulties in the way of securing an effect from these remedies administered internally.

It remains to review the results of the examinations of the urine as regards the diseases. Directing attention to these, we will notice each disease under a distinct head.

Ascites Dependent on Cirrhosis.—Of the four cases of ascites, in two (cases 1 and 7) the disease proved fatal, and in two (cases 5 and 10) there was sufficient improvement for the patients to report themselves able to leave the hospital, and return to labor. In case 1 the quantity of urine rarely exceeded the limit of the healthy average, and generally fell below the average of health; the amount of the solid constituents, on the other hand, generally exceeded the healthy limit. To increase the solids in the urine is not a rational indication in the treatment of dropsy; and in this case the diuretics scarcely diminished the peritoneal effusion, nor was the condition of the patient improved by them. On the contrary, that they were hurtful is ren-

dered probable by the fact that the condition of the patient decidedly improved after these remedies were discontinued, and the citrate of iron and quinia substituted.

The patient, in fact, was sufficiently improved to wish to leave the hospital, and try to work, but he was compelled speedily to return. As I have said under the head of the remarks appended to this case, it affords no encouragement for the use of the nitrate of potassa in the treatment of ascites dependent on cirrhosis.

The case, however, shows that to a certain extent a diuretic effect, as regards the quantity and solids, may be induced by diuretics given internally, notwithstanding the difficulty in the way of the introduction of the remedies into the general circulation arising from the hepatic obstruction.

In the other fatal case, (case 7,) the external use of diuretics was accompanied by a decided increase of the quantity of urine and the amount of solids, and the nitrate of potassa occasioned a still greater increase; the quantity and solids, however, falling below the averages of health. No apparent influence was exerted on the progress of the disease; the rapid and great accumulation of effusion rendered tapping necessary at short intervals, and he left the hospital to die, having an apprehension of being examined after death if he died in the hospital. This case also proves that the existence of cirrhosis does not prevent the operation of diuretics to a limited extent, although, like case 1, it affords no encouragement to expect benefit from their use.

In case 5, the dropsy had already diminished considerably before coming under my observation. The nitrate of potassa increased but slightly the quantity of urine, but the amount of solids was considerably augmented. The patient improved during the ten days that this remedy was given, but he improved still more rapidly when taking the citrate of iron and quinia, which were continued till he left the hospital.

In case 10, the nitrate of potassa, given for nine days, produced a moderate increase of the quantity of urine, and a more marked augmentation of the solids. The ascites diminished under this treatment, but the progressive improvement was as marked, if not more so, during the following twenty-seven days, up to the date of his discharge, during which period the patient was taking the citrate of iron and quinia.

The results in these four cases do not seem to afford any evidence of the value of diuretics in the treatment of ascites dependent on

cirrhosis. They show that the quantity of urine may be moderately increased by these remedies in this disease, but that the solids are augmented out of proportion to the increase of quantity; and they render probable the conclusion to which theoretical considerations lead us, viz.: that the augmentation of the solids of the urine in this disease is more likely to injure than benefit the patient.

Albuminuria dependent on Bright's Disease.—Of the three cases following under this head, in two the disease proved fatal. In one of these, (case 2,) the iodide of potassium was the only diuretic remedy given. This produced no apparent effect. In the other fatal case, (case 8,) the external use of diuretics appeared to increase somewhat the quantity of urine, and still more the amount of solids. No apparent effect on the condition of the patient was observed. Subsequently, digitalis, squill and juniper produced no diuretic effect, and the same is true of the nitrate of potassa. In case 9, digitalis, squill and juniper, given internally, occasioned a marked increase in the quantity of urine, and an equal increase in the amount of solids. A still more marked effect on both the quantity and solids was occasioned by the nitrate of potassa. The patient's condition when taking these remedies progressively improved as regards the general dropsy, and in all other respects. These remedies were continued for nineteen days. The patient remained in hospital forty days after the diuretic treatment was discontinued. The dropsy did not return, and he continued progressively to improve until he felt able to leave the hospital; the urine, however, being albuminous at the time of his discharge. So far as a single case goes, this furnishes evidence that diuretics may be sometimes usefully employed in general dropsy dependent on Bright's disease.

Subacute Rheumatism.—Of the three cases, in all the acetate of potassa was the diuretic remedy employed, this being selected in consequence of its value being so highly extolled by Dr. Golding Bird. In case 3, the diuretic effect of the remedy was apparent in an increased quantity of urine and the amount of solids; but in this case, both the quantity and solids became still more increased after the remedy was discontinued, and no remedy was taken. The improvement while the patient was taking the diuretic remedy was marked. The acetate of potassa constituted the sole treatment in this case. The affection had existed for fourteen days before admission, and the patient was discharged quite well, after remaining in hospital twenty-four days.

In case 4, the quantity of urine and the amount of solids were greatly over the averages of health while the patient was taking the diuretic remedy. The remedy was continued for ten days. He was

in hospital seventeen days, and the affection had existed for fifteen days before his admission. The improvement in hospital was progressive, and he was free from rheumatism when discharged.

In case 6, the quantity of urine and the amount of solids were increased while the diuretic remedy was given, but the quantity of urine still fell below the average of health, and the amount of solids but slightly exceeded the healthy limit. In this case, both the quantity and solids were increased after the acetate of potassa was discontinued, and twenty grains of the sulphate of quinia given daily. But subsequently the wine of colchicum, given for five days, produced a marked increase in the amount of solids. The patient had been affected with rheumatism for four weeks before his admission. The rheumatic affection was relieved after the patient had been in hospital twenty days, the treatment during this period having consisted of the acetate of potassa for five days, the sulphate of quinia for eight days, and the wine of colchicum for five days. Mercurialization was afterwards resorted to, and fifty-four days after the date of his admission he was nearly free from rheumatism.

The rational indication to be fulfilled by diuretic remedies in rheumatism relates to the increase of the solids of the urine. These three cases appear to afford evidence of the efficient operation of the acetate of potassa in this regard. They also render highly probable the conclusion that this effect exerted a favorable influence on the disease.

In concluding this paper, I would repeat that I claim for the researches which it embraces, importance only as a small contribution to the study of the effects of diuretic remedies, and as indicating a direction in which clinical observation may be prosecuted with the expectation of reaching conclusions which will be highly useful in their bearings on practical medicine. A large number of recorded histories of cases of different diseases, under different measures of treatment, including daily examinations of the urine as regards not only quantity and specific gravity, but the different substances which make up its solid constituents, would afford materials for analytical investigation which could hardly fail to lead to valuable conclusions. To pursue this plan of study sufficiently to accumulate a large stock of cases, would require much patience and perseverance, in addition to the other qualifications for clinical observation. The plan could be successfully carried out only in a large hospital where all the necessary facilities are afforded, and by observers who, aside from competency for the task, are able to devote to it a large portion of their time for a series

of months or years. Should this paper chance to enlist in behalf of such an undertaking the interest of any one or more zealous young physicians who lack only the proper field of study, I will engage that this obstacle shall be removed.

GRAMERCY PARK HOUSE, *August* 24, 1860.

Lectures on Displacements of the Uterus. By E. R. PEASLEE, M.D., LL.D., Professor of Obstetrics and Diseases of Women and Children in the New York Medical College.

LECTURE VII.

GENTLEMEN—I to-day speak of the anterior displacements of the uterus—anteversion and anteflexion.

The existence of anteversion was first verified by Levret, but not till after the death of his patient. Mistaking the projection into the bladder of the fundus uteri displaced against its posterior wall, for a vesical calculus, he performed the operation of lithotomy; when, to his astonishment, no stone was found in the bladder. The patient died in consequence of the operation, and the *post-mortem* examination revealed an anteversion of the womb.

Retroflexion was not recognized as distinct from retroversion for many years afterwards; and even now by some writers the word retroversion is made to include both displacements. The distinction is not so indispensable in a practical point of view, as we have shown that between retroversion and retroflexion to be; but it should nevertheless be constantly kept in view. They are not any more than the posterior displacements, merely different degrees of the same thing; and anteversion, like retroversion, most frequently follows parturition, either premature or otherwise; while anteflexion is, and even more than retroflexion, most frequently independent of it.

1. In *anteflexion* the body only of the uterus falls forward, the cervix still preserving its normal position in the pelvis; the point of flexion being almost always at the junction of the body with the neck, (Fig. 9,) for reasons specified in Lecture V., page 19.

2. In *anteversion* the whole uterus is displaced forward, but without any flexure of the body upon the neck; so that it comes to assume an antero-posterior position in the pelvis, the fundus pointing forward, (Fig. 10.)

But the distinction between the two displacements is so similar to

that between the two posterior displacements, (Lect. V., pp. 20, 21,) that I need not dwell upon them here.

Since the body of the uterus naturally inclines in front of a vertical line touching the posterior surface of the cervix, (Fig. 1, Lect. I., p. 166,) we need recognize but two degrees of each of these displacements. In the *first* degree of each the body of the uterus falls for-

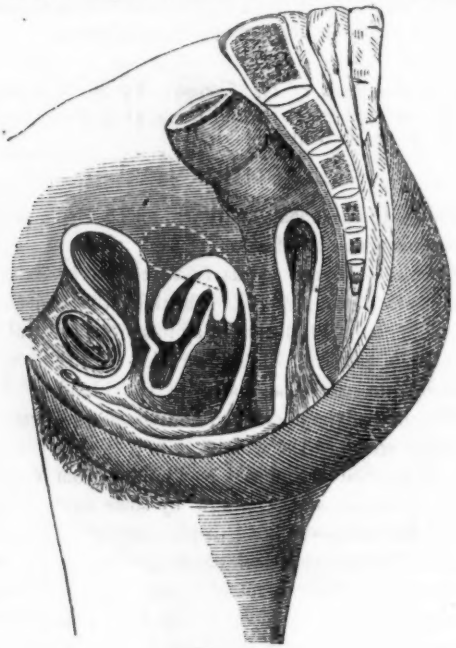

FIG. 9.

Anteflexion.—The womb is seen in the state of complete anteflexion—the second degree, or complete anterior reduplication; it falling into a cavity between the anterior wall of the vagina and the posterior wall of the bladder. The dotted lines show the position of the body of the uterus in the first degree of anteflexion. Neither the uterus itself, nor the dotted lines, are placed sufficiently low; and the rectum being represented as distended, the position of the first degree of anteflexion appears too nearly coincident with the normal position of the uterus. Generally, in anteflexion, the uterus is inclined to one side, and the bladder to the other.

ward upon the posterior wall of the bladder, (if somewhat distended,) so that its axis prolonged would fall below the middle point between the umbilicus and the symphysis pubis. In case of *anteflexion*, this would cause a bend of the body upon the cervix at about a right angle. In the *second* degree of both, the fundus points towards the symphysis pubis. In *anteversion*, it crowds the posterior wall of the bladder forward into its cavity; while in *anteflexion*, it falls down be-

tween the anterior wall of the vagina and the neck of the bladder, (after stretching the areolar tissue connecting them,) and thus a complete reduplication of the uterus anteriorly ensues.

The dotted lines in the accompanying figures represent the first degree of each. In *retroversion*, the neck of the uterus is also displaced backward, looking, in the second degree, towards the hollow of the sacrum; while it is not displaced at all in anteflexion.

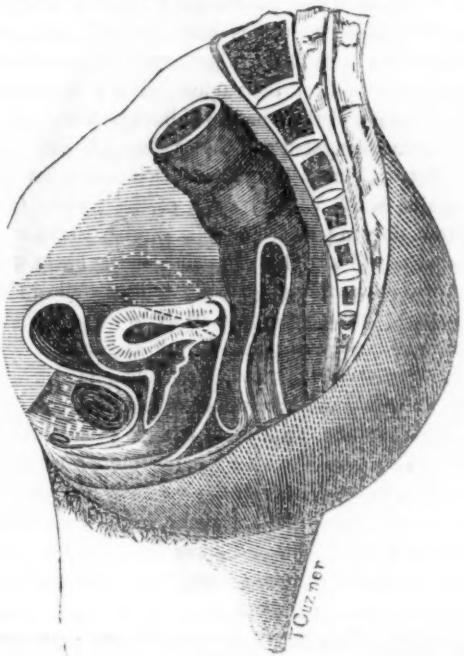

FIG. 10.

Anteversion, showing the first and second degrees. The uterus should be represented as falling down completely in contact with the bladder and the posterior wall of the vagina. Dotted lines as in preceding figure.

We must, however, admit, as in case of the displacements backward, every possible grade of malposition between the first degree of anteversion and complete anterior reduplication. We also very often find some degrees of prolapsus accompanying anteversion, and, rarely, anteflexion also.

If we inquire into the relative frequency of the anterior and posterior displacements, I should say, that in my observation, the latter

are the more numerous. In those who have not borne children, I have, however, found anteflexion rather more frequently than retroflexion, though Dr. Meigs speaks of the former as very seldom occurring. Both anteversion and retroversion are rare in this class of patients. As occurring after parturition, (premature or otherwise,) anteversion is more common than retroversion or anteflexion; but perhaps not more common than retroflexion.* And I think the reason of this may be found in the anatomical relations of the uterus. A cause acting to displace the uterus *backward*, will, the parts being in the normal state, be more likely to produce a flexion than a version, because the anterior wall of the vagina being firmly connected with the bladder, the body of the womb will bend on the neck before the vagina will become elongated and the os elevated anteriorly, (Fig. 6,) to constitute retroversion. Thus retroflexion will be far more common than retroversion in the natural state of the parts. On the other hand, a force acting to produce *anterior* displacements in the natural state of the parts will also be somewhat more likely to produce a flexion than a version; though, if the posterior wall of the vagina becomes elongated, (and it is far more likely to do so than the anterior in the opposite circumstances, since it is longer, and more easily distended,) anteversion will occur. Anteflexion is, therefore, on the whole, more common than anteversion, while the parts concerned are in their normal condition. But in the relaxed and distensible condition of the vaginal walls ensuing after parturition or miscarriage, either wall may yield, and thus either of the versions may ensue; but since the body of the uterus naturally inclines forward, and the posterior wall of the vagina still yields more readily than the anterior, anteversion more frequently occurs as a sequel of parturition.

Valleix found that of 68 displacements 35 were anterior, (24 of anteversion and 11 of anteflexion;) and 33 were posterior, (12 of retroflexion and 21 of retroversion.) Thus the cases of anteversion were the most numerous, of retroversion next, and those of retroflexion and anteflexion about equal, (12 and 11,) and about one-half as numerous as the two versions.

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* Dr. Meigs, however, up to the time of his publication of Colombat's work, had seen but a single case of anteversion. He has met with several since, (p. 240,) though in more than thirty years he has had but few opportunities of observing it, (p. 234.) He merely alludes to the flexions of the uterus as very rare and unimportant, (p. 238–40.) Mad. Boivin remarks, that these affections (the flexions) are "very rare, and consequently misunderstood," (p. 106.) It would, I think, be more correct to say they are misunderstood, and therefore assumed to be very rare.

Dr. Peebles regards anteversion as a very rare disease, when compared with retroversion. Dubois, on the other hand, thinks anteflexion to be more frequent than retroflexion, as my own observation would decide. M. Boulard has made some interesting observations bearing on the question of the frequency of this malposition. He finds that in the fœtus the uterus is almost always anteflexed; and of 27 adult females who had never borne children, 13 girls from 2 to 13 years old, and 17 full-timed fœtuses, *a majority had anteflexion.* Churchill admits that anteflexion may occur in the non-pregnant state; but he believes such cases to be very rare. During the first three months of pregnancy, anterior displacement of the uterus is so very common that we may almost regard the first degree of anteflexion as the normal position of the uterus at this time. And it often passes into the second degree. Anteversion may also exist during this period, and except in the first pregnancy, it is scarcely less common than anteflexion. Either of these malpositions is, however, rectified as pregnancy advances; and the symptoms it had produced (irritation of the bladder being usually the most troublesome) are thus spontaneously removed. While, therefore, the posterior displacements during pregnancy are of very great, those now under consideration are of comparatively slight, practical importance, (Lect. VI., pp. 127–32.)

The *mechanism* of the anterior displacements is easily understood. In case of *anteflexion*, we have only to assume the action of a force from above downward and forward sufficient to overcome the action of the broad ligaments, (Lect. I., p. 166,) and the natural rigidity of the cervix uteri at its junction with the body of that organ; while in *anteversion* the broad ligaments give way in the same manner, and the posterior wall of the vagina is at the same time elongated upward and backward. And when we remember the natural inclination of the uterus forward, we may well be surprised that these displacements are not more common than they are actually found to be. The round ligaments and the utero-rectal are not necessarily implicated in the anterior displacements; though in cases of long standing both will become contracted, and thus resist attempts to replace the womb, and for a time cause the womb to relapse into its previous position on being replaced. Dr. Meigs discovers in the mechanism of the anterior displacements only a shortening of the round ligaments, which pulls the uterus forward. Churchill states that so long as the bladder contains much urine, this accident (anterior displacement, and especially anteversion,) "may be considered impossible." If this were the fact, we could hardly account for a permanent anterior displacement ever

occurring at all; since in most persons the bladder is moderately distended the greater part of the time. He overlooks the fact that the bladder in these cases is usually inclined to one side of the middle line, and the displaced uterus to the other; and Dr. Meigs has, I think, assumed as a cause what is, in the great majority of instances, merely a sequel of the displacement.

Causes of the Anterior Displacements.—The *direct* causes of anteflexion and anteversion include all agencies which carry the body of the uterus forward and downward, and may be either uterine or extra-uterine. (1.) The former are congestion or inflammation of the body of the uterus, (or of the anterior wall alone;) a fibrous tumor, in the latter, or in the posterior wall, if large enough to crowd the whole uterus forward; polypus uteri, and hypertrophy from previous inflammation, or from arrested involution after parturition and early pregnancy, as before explained. (2.) The extra-uterine are pelvic tumors, ovarian or otherwise, crowding the uterus forward, and an habitually loaded and distended condition of the sigmoid flexure of the colon, producing the same effect. An obstinate diarrhœa has been known to produce it. So also have violent efforts, as in vomiting, difficult defecation, laughing, coughing, and riding in an uneasy carriage. A blow on the abdomen, a fall, or rising too soon after parturition, may also produce an anterior displacement at once.

Dr. Meigs admits the agency of some of the preceding causes, but proceeds to remark: "It is still clear to me that a contraction of the *ligamenta rotunda* is much more generally the cause of an anteversion." (p. 235.)* From this doctrine I have already expressed my dissent. We must, however, admit that a shortening of the round ligaments as a sequel to inflammation implicating them, of the appendages of the uterus or of the contiguous peritoneum, may gradually occur. And *if* such a change occurs, anterior displacement must of course result. So, also, adhesions of the cervix uteri to the posterior wall of the vagina may cause anteversion, as in a case communicated by Mad. Legrand. But all such should be regarded, I should say, as exceptional cases.

The *predisposing* causes are relaxation of the broad ligaments, or increased weight of the uterus, from any cause, and especially from parturition, (premature or at full term,) or in connection with menstruation. Certain occupations, also, predispose to anterior displacements. I have found sewing-girls to be peculiarly liable to anteflexion.

* See also further on, p. 274.

Certain natural predisposing influences should also be added; as a long, much curved, and strong vagina, and a high and deeply curved sacrum. The convolutions of the small intestine normally surround the uterus, and support it equally on all sides; but if it lies high, it will, with its natural inclination forward, be more liable to have a greater mass of intestines behind it, and when the bladder is empty, the womb readily yields to this pressure. Thus the fundus curves forward, while the os points backward into the deeply excavated sacrum, and anteversion is effected. Both the anterior displacements are usually produced gradually; and when once an undue inclination forward is given to the fundus, a slight agency constantly acting will in time complete the malposition.

Symptoms of the Anterior Displacements.—Either anteversion or anteflexion in the first degree, or if gradually induced, may exist without any local symptoms; though many of the general symptoms mentioned in Lect. III. (p. 427–9) may be produced, and especially those affecting the digestive and the nervous system. Very much depends here, as in the other displacements, upon the temperament of the patient. But, as a general rule, the local symptoms of anteversion are more decided than those of anteflexion; there being more displacement of the appendages and surroundings of the womb, and more pressure upon them, in the former than in the latter. Still, the symptoms of neither are distinctive, but are those mentioned as common to several uterine ailments, in Lect. I., (p. 168.)

Anteversion is also more likely to be induced suddenly than anteflexion, as the former implies a more relaxed and readily yielding state of the parts concerned; and herein is another reason for the greater severity of its symptoms. Anteflexion, however, when produced by a violent effort, may be attended by equally severe symptoms, and only an internal examination can distinguish them.

Of *anteversion* in the second degree, the most common symptoms are the following: Vesical tenesmus and frequent micturition; perhaps, also, rectal tenesmus, and especially during the catamenial period; a bearing-down sensation; pain in the lumbar and inguinal region, and down the limbs; sometimes a feeling of weight in the rectum, from direct pressure against it of the cervix; and constipation from torpor of the muscular coat of the alimentary canal. All these unpleasant sensations are increased during the menstrual period, and by standing and walking; and diminished while in a recumbent position. Generally leucorrhœa also exists, and menstruation is abnormal.

The symptoms of *anteflexion* in the second degree are usually less pro-

nounced; it usually having been gradually produced, and having existed a long time before being detected. In almost all cases, however, we find sterility, dysmenorrhœa, and ovarian irritation. Dysmenorrhœa is due primarily to the partial closure of the canal of the cervix at the point of flexion. Ovarian irritation depends usually on the congestion (or inflammation) resulting from the displacement of one or both ovaries, in connection with that of the uterus; but this also becomes an additional cause of dysmenorrhœa.

Sterility also is often the result of anteversion, but not so generally as of anteflexion, as might be supposed.

The *physical* signs of anterior displacements are alone conclusive, as is the case with the posterior.

The *first degree* of anteversion or anteflexion may afford no symptoms, and may therefore, in most cases, escape detection. In the former, however, there will be found a backward displacement of the cervix on making a vaginal examination; but the sound alone will generally give evidence of the latter.

In the *second* degree of *anteversion* a vaginal examination shows that the uterus lies horizontally in the pelvis, the os pointing back towards the rectum, and the fundus forward against the neck of the bladder; consequently the curve of the vagina is changed, its upper extremity extending too far backward. Often the finger may trace the whole length of the uterus on its anterior (now its inferior) surface. Frequently the os uteri is carried so high posteriorly as with difficulty to be reached by the finger; but the sound is very seldom required to verify complete anteversion.

In the *second* degree of *anteflexion*, or complete anterior reduplication, the os and cervix are found in the normal position in the vagina, though the upper extremity of the latter is often carried a little backward, while the fundus is felt as a firm, rather tender, rounded tumor, lying between the anterior wall of the vagina and the bladder. Sometimes, however, the fundus is quite insensible to pressure, and thus might easily be mistaken for a fibrous tumor in the anterior wall of the uterus; besides, since only the anterior wall of the vagina intervenes between the cervix and the fundus, the latter may at first seem to be structurally continuous with the latter, and thus further confirm the suspicion of a fibrous tumor.

Per rectum, the os uteri may be felt projecting backward in the second degree of anteversion; but no positive knowledge is acquired in this way in case of anteflexion.

But if any doubt remain, the *uterine sound* decides the diagnosis.

In using this instrument, in case of *anteflexion*, we introduce it with the concavity upward, as if the uterus were in the normal position, the patient lying on the back, as directed in Lect. V., (p. 26;) and when the bulb is arrested on reaching the point of flexion, the handle is depressed, and thus the instrument passes into the proper cavity of the organ. The process by which the body of the uterus is then elevated to its normal position, of course needs no explanation. The same manipulations may be required to pass the sound into the uterine cavity, as those specified in connection with retroflexion, (Lect. V., p. 26,) and the tumor will of course no longer be felt after the sound is introduced.

Anteversion or anteflexion occurring in early pregnancy often presents no special symptoms, aside from the accompanying signs of pregnancy. Generally, however, there is some suffering from pressure upon the bladder. The diagnosis is here, of course, to be made out without the use of the sound.

Diagnosis of Anteflexion and Anteversion.—*Anteflexion* of the unimpregnated womb is distinguishable from the following conditions, as follows: anteversion is not so likely to be mistaken for other conditions, since the position of the os uteri at once suggests it.

1. From *fibrous tumor in or inflammations of the anterior wall of the uterus.*—The fundus uteri is usually tender, tumors usually not so; but the sound decides in both cases. This localized inflammation, however, but rarely occurs.

2. From *early pregnancy.*—All the signs of pregnancy are wanting; therefore the sound being used, decides. If there be doubt as to pregnancy, wait; at any rate, do not use the sound.

3. From *post-partum hypertrophy.*—Sound shows the uterus is *in situ*, but enlarged, and its cavity elongated.

4. From *carcinoma of the uterus.*—Carcinoma almost always invades the cervix uteri first; but use the sound.

5. From an *ovarian* (or other pelvic) *tumor.*—The sound isolates the uterus from the tumor.

6. From *extra-uterine pregnancy.*—Here again the sound isolates the uterus, if in its normal position, from the extra-uterine mass.

7. The anteflexed uterus sometimes inclines to one side, and may be mistaken for a *displaced ovary*. The sound decides.

8. *Anteversion* has been mistaken for *calculus in the bladder* by Levret and others; but the fundus uteri felt by the sound in the *bladder* is not so solid as a calculus, (unless encysted,) and does not give the peculiar sound on being struck; but the uterine sound decides.

9. Lastly, *Anteflexion* is distinguished from *anteversion*, with which it has so generally been confounded, by the difference in position of the cervix, as explained on page 274.

Effects and Complications of Anteflexion and Anteversion.—Congestion of the anterior wall, and sometimes of the entire uterus, is a very constant effect of the anterior displacements. Ovarian irritation also exists, and dysmenorrhœa, especially with anteflexion. Sterility also is more frequently associated with the latter; but not seldom with anteversion also, though not a necessary effect of either. Sexual desire is also diminished in both, but perhaps more decidedly in cases of flexion. Atrophy of the cervix at the point and on the side of flexion is very common in cases of anteflexion of long standing; but it sometimes presents the reverse condition, and hypertrophy (from previous inflammation) ensues. Sometimes the hypertrophy affects the entire organ. Adhesions to the bladder and the contiguous peritoneal surfaces occur, though very seldom, in either of these displacements, and which render replacement of the uterus impossible. The round ligaments may also gradually become contracted to such an extent as to afford the same obstruction to reposition.

Prognosis of Anteflexion and Anteversion.—The anterior are less serious, if neglected, than the posterior displacements. Anteversion, (or anteflexion,) occurring in early pregnancy, will be spontaneously rectified as gestation advances to the fourth month.

In either of the anterior displacements of the unimpregnated uterus, we cannot promise a complete cure, *i. e.*, a permanent restoration of the uterus to its normal position, within any precise period, or without the employment of some form of intra-uterine support. We may, however, expect to afford relief in every case, and if opportunity is afforded by the circumstances of the patient, to remove all the symptoms in most. We must therefore not promise our patients too much, but candidly explain to them at the outset what we may rationally expect to accomplish. If pregnancy supervene, a cure may be expected from skillful management after parturition; though the best management will not always prevent a relapse.

In case atrophy of the cervix anteriorly has occurred, we may not succeed by any method of treatment in permanently restoring the uterus to its place. Even in these cases, however, the symptoms may be removed, and not seldom also the sterility dependent on the malposition. Our prognosis should, however, be guarded, since we can never ascertain the precise amount of atrophy existing in any particular case.

Treatment of Anteversion and Anteflexion.—In discussing the treatment of the anterior displacements, I shall speak of the different topics in the order already adopted for the treatment of the displacements backward; that both the similarity and the contrasts may be made the more apparent.

I. Treatment of the Anterior Displacements during Pregnancy.

It has been shown (p. 271) that, generally, anteversion is far more common in early pregnancy than anteflexion; while in first pregnancies anteflexion is more common than anteversion. While, also, the posterior displacements during early pregnancy are of serious import, and require prompt and efficient treatment, the anterior are not so, and often require no treatment at all; being spontaneously relieved after the beginning of the fourth month. If, however, much inconvenience results during the first three months, a recumbent position during several hours each day may be advised; or a simple ring pessary if the patient's circumstances do not allow of such repose. Of course the effects of the instrument are to be watched. I have, however, never witnessed the mischievous effects of pessaries in such circumstances, of which I have read so much. Frequently the principal malaise in such cases results from a slight degree of descent accompanying the anterior displacement; and if so, the pessary may give entire relief. At all events, if the patient is more comfortable with the instrument than without it, we need not fear that the danger of miscarriage is increased by its presence. Of course it is to be removed as soon as the uterus rises above the superior strait of the pelvis.

II. Treatment of the Anterior Displacements of the Unimpregnated Womb.

Anteversion of the unimpregnated uterus is more frequent than anteflexion only in those who have had children, (or a miscarriage;) the causes of anterior displacements producing version rather than flexion when the vagina is rendered lax and easily distensible by re cent parturition. In others, unless a tumor exist to produce anteversion, anteflexion is altogether more common.

Here, as with the posterior displacements, I should first call attention to the cases suddenly induced. These are almost always cases of *anteversion*, and occur in one recently delivered or immediately after menstruation, in consequence of some violent effort. Occurring in any other circumstances, we should suspect the existence of early pregnancy; and unless we can be very positive it does not exist, the case is to be treated as belonging to the class first considered. If

pregnancy does not exist, the womb is to be replaced, (and usually it can be done without the use of the sound,) and the patient is to be kept lying on the back for a few days, (a week at least.) Attitude is the surest safeguard against a relapse in such cases. A ring pessary is to be worn for a time after she is allowed to sit up and walk; and cold vaginal injections will be found useful in restoring the tone of the vagina.

But *anteflexion* of the non-pregnant womb is almost always gradually induced, and anteversion is not often brought on suddenly; and though some cases present no symptoms, and therefore require no treatment, we expect, as with the posterior displacements, to be obliged to resort to both local and general treatment.

In the way of *general* treatment, however, nothing peculiar is required in this class of malpositions. I therefore refer to my remarks on this subject in Lect. IV., (p. 529,) and here confine myself to the local management of these cases.

Of the *local treatment* of the anterior displacements, the indications are, as of the other displacements, three-fold:

1. To remove the cause of displacement, and certain complications, if still existing and in action.
2. To replace the uterus.
3. To retain it in position.

There is as much skepticism in some quarters in regard to the value of local treatment of the anterior as there is in regard to that of the posterior displacements, and to which I have alluded in the preceding Lecture, (p. 133.) I, however, have no controversy with such; nor with those who, on the other hand, maintain that the treatment of these cases is a very simple matter. I consider the local treatment, as a general rule, entirely indispensable; and shall proceed at once to specify the means I have found the most successful, and therefore consider the most reliable.

1. *Remove still active causes or complications.*—It by no means follows in anterior displacements, any more than in the posterior, that still active causes and complications are always to be removed before the uterus is replaced. In fact, I have generally replaced the organ at once, and then given attention to any complication requiring it. But the question first arising is, whether the uterus is to be replaced at once, or whether some preliminary treatment is previously demanded. Of course, a distended state of the bladder and rectum are to be relieved before the uterus is reposited; and in some cases, any inflammation or decided congestion of the uterus should be previously diminished, at least. Ovarian irritation or inflammation calls still more de-

cidedly for attention before the reposition is attempted; and especially if the use of the sound is necessitated to effect replacement. Still, it should be remembered, that all these causes and complications are more promptly relieved after the uterus is restored to its normal position; and it is only in a comparatively very small number of cases that I should not decide to replace the womb at once, and then attend to the complications mentioned. There is, moreover, usually less risk in doing this in the anterior than in case of the posterior displacements; but no probable risk should be incurred in any case.

The special means to be adopted for the abatement of the conditions I have mentioned have been specified in the fourth Lecture, (p. 525–6,) and to that I refer to avoid repetition.

2. *Replace the Uterus.*—To effect this, the patient may lie upon the back with the pelvis raised, as directed in Lecture V., (p. 26); and in case of *anteversion*, the cervix may be brought forward by means of the fenestrated spoon, as recommended by Mad. Boivin, or by the looped wire described in the last Lecture, (p. 129.) A long index finger may, unaided, accomplish this; or, these means failing, the uterine sound passed an inch into the canal of the cervix may bring the os into position. Since the womb is not flexed, the fundus will pass upward and backward as the cervix comes forward, though we cannot probably carry it higher in this way than into the position of the first degree of anteversion. In case of *anteflexion*, we may also sometimes elevate the fundus uteri to the first degree of the displacement, by pressing against it with the index finger in the vagina. But neither in anteflexion nor anteversion can we with certainty restore the uterus to its normal position by any other means than the use of the uterine sound.* It is to be used as before directed, (p. 275.) In cases of complete anterior reduplication, it is often very difficult to pass the sound beyond the point of flexion, except by a procedure similar to that recommended in Lect. V., (p. 26.) The handle of the sound should be much depressed between the thighs of the patient; when, if the left index finger is used *per vaginam*, to elevate the fundus uteri, the instrument is usually carried by the right hand into the uterine cavity without further delay.

3. *Maintain the uterus in position.*—In these displacements, as in retroversion and retroflexion, the womb—except in rare instances of anteversion suddenly induced—returns nearly if not precisely into the position it before occupied, instantly, or at most a few minutes, after the sound (or other force which replaced it) is withdrawn. Here,

* Previously to the invention of the latter, by Prof. Simpson, it had been proposed to attempt reposition in these cases by means of a sound in the bladder.

therefore, as in the other displacements, we must resort for a time to a mechanical support for the uterus—to some form of pessary.

What kind of pessary shall we adopt, is therefore the important inquiry. Nauche advises a cup-and-ball pessary; Simpson and Valleix a stem pessary; but Churchill thinks such means will seldom be necessary, and may be injurious.* Dr. Peebles uses Hodges' pessary, with a transverse bar in front; on the assumption that if the vagina is "supported to its proper length and height, displacement will be rectified." But he admits, very correctly, that a long vagina, and, of course, high at its upper extremity, is a predisposing cause of these displacements. Can we then "rectify" an anteflexion, when once produced, by again bringing to bear upon the uterus the condition which at first predisposed to its production? Dr. Meigs recommends the globe pessary, two and a half inches in diameter, in case of anteversion; though he says that an ovoid pessary, as large as a Normandy hen's egg, passed into the vagina with the smaller extremity uppermost, and pressing against the fundus uteri through the anterior vaginal wall, is still better in anteflexion. The former, he remarks, "will lift the uterus very high in the pelvis. Its action must be to push the womb upward and backward."† But merely elevating the uterus does not make it straight (or remove the bend) in a case of anteflexion, any more than carrying up stairs a man who has a fracture will reduce the latter—nor bring an anteverted womb into its natural position; for in both cases the womb is lifted *as a whole*, and almost exclusively by a force applied to the cervix. But Dr. Meigs' object in thus lifting the uterus is to elongate the round ligaments, of which he believes a contraction to be the cause of the anterior displacements in the vast majority of cases. *If* such a contraction actually exists, I should consider the globe pessary the best appliance for gradually elongating them, preparatory to replacing the uterus with the sound. But we actually find in at least nine cases out of ten of the anterior displacements, that there is no obstacle to replacement by the sound, except such as is afforded by the mere weight of the body of the uterus; and thus *demonstrate that no such contraction of the round ligaments exists.* Let us, then, prove the existence of such contractions, as we always may by the sound if there be any, before we base our practice upon it. Having detected it, we may very rationally use the globe pessary for a time; and if there be ground for the belief that the contraction results from inflammation or spasm, the seton

* Diseases of Women, p. 323. † Woman and her Diseases, p. 237.

over the pubes, or ointments there applied, as suggested by Mad. Boivin, may rationally be resorted to.

You will feel obliged, I think, on again referring to my first lecture, (Fig. 1,) to come to the conclusion that an instrument introduced into the vagina merely with the intention of rectifying either anteflexion or anteversion, must rise as high as to the middle of the anterior surface of the body of the womb, in order to retain the latter even as high as the position of the first degree of these displacements; and that such a position of the upper portion of the instrument is anatomically impossible. The like cannot be effected even in case of the posterior displacements, as we have seen, (Lect. VI., p. 135,) though the posterior wall of the vagina is longer and far more distensible than the anterior. Sometimes, indeed, the anterior vaginal wall is so firmly attached to the cervix uteri and to the bladder, that a pessary cannot be made to force it upward towards the body of the uterus in the least degree. We may push an anteflected or an anteverted womb beyond our reach, and out of the pale of our cognizance, by vaginal instruments; but we cannot, by their use, either restore the uterus to its normal position, or retain it there after reposition by other means. I should therefore say of these, as I have asserted respecting the posterior displacements—

1. *It is impossible entirely to prevent anterior displacement by any appliance in the vagina alone.*

2. *The only agent which can certainly retain the unimpregnated uterus in place, in case of anteflexion or anteversion, is an instrument entering the uterine cavity,* (an intra-uterine pessary.)

Shall we, then, entirely discard the vaginal pessary in the management of these displacements, and in all cases use the intra-uterine? By no means. But let us understand precisely why we use the former, and what we may expect them to accomplish. They are merely palliative, and not curative. They merely retain the uterus in a somewhat better position than that in which we found it. But they often give complete relief from all the symptoms, and should be used to the exclusion of the intra-uterine instruments, in more than nine-tenths of all the cases.

I therefore consider first the palliative treatment of these displacements, and then their radical treatment.

1. In the *palliative* management of *anteversion*, a simple ring pessary of tin or gutta percha, large enough fully to distend the upper extremity of the vagina, and thus pull the os uteri forward, is to be used. If an ovate form be given to it, as explained in the preceding

Lecture, (p. 136,) the smaller extremity of the instrument may be made to rise as high against the anterior wall of the vagina, and thus raise the fundus as high as the globe or the ovoid pessary. But it is not merely the height to which the fundus uteri is raised by a vaginal pessary that gives it its value; since it can effect but very little, we have seen, in that way. We need not, therefore, be so very strenuous respecting the precise form of the instrument, provided it fits well. It may, however, prove valuable in three ways: (1.) By pulling the os uteri forward, conception may occur in a patient previously sterile. (2.) It keeps the uterus more nearly at rest, and thus gives great relief, if there be irritation from inflammation or congestion. (3.) It relieves the slight degree of descent which often accompanies anteversion, and which not seldom is the main cause of the patient's suffering.

In case of complete *anteflexion*, I prefer a tin instrument, first fixed in the form of a narrow ellipse, and then curved, so that its two sides fall equally below its original plane; and so that when it is introduced into the vagina, the anterior and the posterior portions are higher than the lateral. In these cases it is desirable that the anterior portion of the instrument rise as high as possible in the vagina. For the fundus uteri having, as it were, dissected off the bladder from the anterior surface of its body and the upper portion of the anterior wall of the vagina, has fallen into the cavity thus formed between the bladder and the vagina, as it does into the Douglass *cul-de-sac*, between the rectum and vagina, in complete retroflexion. The instrument should, therefore, rise high enough entirely to obliterate this cavity, if possible, by its pressure of the vaginal wall forward against the bladder, and thus prevent the fundus uteri from again occupying it. And to accomplish this, it must rise higher than the symphysis pubis; a thing usually out of the question, however, in case the vagina is quite short.

Shall we also advise the patient to keep her bladder pretty full (16 to 20 oz.) of urine all the while after reposition, in order to prevent the womb from falling back again? as suggested by a high authority. I confess I have never rendered such advice. In principle it is like advising to keep the rectum full of its natural contents after reposition in case of retroflexion; and besides, it fails to accomplish its object, since the full bladder in such cases usually inclines to one side, and the uterus to the other, as we have seen.

If, from the presence of adhesions, (or of contraction of the round

ligaments,) reposition cannot be effected, I would advise the application of a globe pessary.

In respect to the time during which the pessary should be worn, and other associated particulars, I again refer to the preceding Lecture, (p. 137.) Levret thought it should be worn 12 to 15 months; "when the leucorrhœa it caused at first ceases, and constitutes a sign of recovery." Désormeaux thought it might be removed much sooner than at the end of the time just mentioned. Here again I refer to Lect. IV., (p. 528.)

2. The *radical treatment* may, on the other hand, be resorted to in a small minority of cases of the anterior displacements. The circumstances in which intra-uterine instruments are here advisable, are essentially the same as those mentioned in the preceding Lecture, (p. 138.) They are contra-indicated by the presence of congestion or inflammation of the uterus or the ovaries, or irritation of either from any cause. Intra-uterine supports are, however, appropriate far more frequently in cases of complete anteflexion than of anteversion; both because the palliative method already explained, though very beneficial in anteversion, is far less satisfactory in anteflexion, and because the radical treatment is usually much better tolerated in anteflexion than in anteversion, or in either of the posterior displacements.

The patient is to be prepared for the use of the intra-uterine instrument in these cases, as in those described in the preceding Lecture, (p. 138,) by the daily introduction of the uterine sound. Less time is, however, usually here required, before the intra-uterine pessary can be used, than in the other class of cases, and it is almost always worn with less inconvenience. I have treated cases of anteflexion in which not the least inconvenience was experienced, (save from a slight leucorrhœal discharge,) after its first introduction.

So long, however, as I used Simpson's intra-uterine pessary, and Valleix's modification of it, I could not have said what I have just asserted. In cases of anteflexion or anteversion, attended by a lax and distensible condition of the vagina—and which is quite common in anteversion, for reasons before mentioned—I have often used the instrument described in the preceding Lecture, (Fig. 8,) with very satisfactory results. But in case of anteflexion in those who have never been pregnant, and in whom the vagina is narrow, and still retains its natural tone, I have found a more simple instrument, which I devised some three years since, to answer equally well. (Fig. 11.)

It is made of pure silver, and consists of a stem, (a tube,) $2\frac{3}{4}$ inches to 3 inches long, and $\frac{3}{16}$ inch in diameter, passing centrally

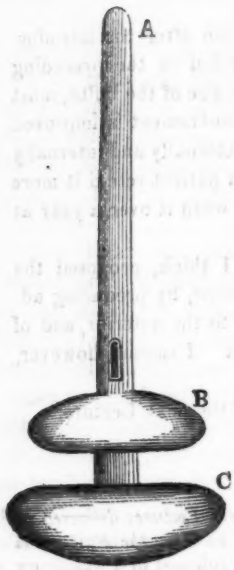


FIG. 11.

Instrument for the radical treatment of anteflexion. A the stem, or intra-uterine portion; B the upper flattened bulb, supporting os uteri; C the lower bulb, resting on posterior wall of vagina. The stem is drawn too large.

through two flattened bulbs. The smaller of them, about $\frac{5}{8}$ of an inch in diameter, is placed about $2\frac{1}{4}$ inches from one end of the tube; this free portion of the tube being the stem or intra-uterine portion of the instrument; while the other bulb, one inch in diameter, is placed at the other end of the tube. When the instrument is introduced, the smaller bulb supports the os uteri, while the larger (one-half inch below the other) rests on the posterior wall of the vagina, above the levator ani. It is passed into the uterus on a staff, as is the stem of the instrument described in the last lecture. A silk thread may be attached to it, so that the patient may remove it at any time, if this become necessary.

You might suspect that so simple an instrument would soon fall out of the uterus if the patient is allowed to walk about. But if the vagina retains its normal tone and dimensions, and the diameter of the lower bulb be well adapted to the case, that canal closes so accurately around the bulb and into the space between the two bulbs, that it is retained in place for weeks or months in succession if required. You will understand that I use it in *anteflexion*, rather than in anteversion, and will perceive that the very tendency of the fundus uteri to return after replacement directs the upper end of the stem forward, and, of course, the lower bulb backward, and thus prevents the instrument from leaving the vagina. You would naturally infer, also, that the instrument would not answer the purpose at all in case of *retroflexion*, where the womb tends to fall backward after replacement. And yet, in some cases of retroflexion in unmarried patients, I have found the contractility of the vagina sufficient to retain the lower bulb in place, if of proper dimensions, in spite of its tendency to move downward and forward towards the os externum. I have seldom found a case requiring a bulb more than one and one-quarter inch in diameter, nor one in which it should be less than three-fourths of an inch. If the os uteri is patulous, a thin plate of gutta percha one inch or more in diameter, with a hole through its centre for the stem to pass

through, may be applied over the upper bulb before the instrument is passed into the uterus.

The measures to be adopted if irritation ensue after the introduction of the instrument are the same as specified in the preceding Lecture. The length of the stem, as well as the size of the bulbs, must be adapted to each particular case, and the instrument is improved for a long-continued use by being gilded both externally and internally by the galvanic process.* I have never had a patient retain it more than four months at a time, though some have worn it over a year at intervals.

Finally, I should add that Amussat first, I think, proposed the radical cure of anterior and posterior displacements, by producing adhesion of the cervix uteri, in case of the former to the anterior, and of the latter to the posterior wall of the vagina. I cannot, however, commend this practice.

Inversion of the uterus will be the subject of the next Lecture.

The Physiology of the Circulation. A Course of Lectures delivered in the College of Physicians and Surgeons, New York, in the Fall Term of 1859. By JOHN C. DALTON, JR., M.D., Professor of Physiology and Microscopic Anatomy.

LECTURE X.

(OCTOBER 5.)

Formation of Sugar in Liver after Death—Mode of its Production from Glycogenic Matter—Decomposition of Sugar in the Blood—Increased Production during Digestive Process—Its Accumulation in Disease—Appearance in Urine in Diabetes—Quantity of Sugar Produced and Destroyed Daily—Formation and Discharge of Urea—Decomposition of Fibrin in the Blood—Material Absorbed and Discharged by Respiration—By Digestion and Excretion—Table of Ingesta and Egesta—Entire Quantity of the Blood—Quantity of Fluids Secreted and Reabsorbed—Renovation of Blood in Circulatory Apparatus.

In the last Lecture, gentlemen, we found that one of the most remarkable phenomena of the circulation is the production of the sugar in the liver. The blood coming to that organ by the portal vein contains no saccharine ingredient, in fasting animals, while the blood passing off by the hepatic veins is abundantly charged with it. I told you that this sugar was not formed in the blood itself, but that it was produced in the tissue of the liver, and absorbed thence by the circulating fluid.

* This instrument also is made by Otto & Reynders, 58 Chatham Street.

The proof of this is, that sugar will continue to be formed in the liver, after it is emptied of blood, and separated from the rest of the body.

Here is the liver which you saw yesterday, and which had been drained of blood by a watery injection. At that time a decoction of the organ did not contain any trace of sugar, and did not cause the least reduction of the oxide of copper in Trommer's test. Since then it has remained untouched, and merely protected from the dust by a glass cover.

You see some of the watery fluid has drained away from the organ, and collected at the bottom of the vessel. I will pour off a little of it, and subject it to Trommer's test. On boiling, you observe, the oxide of copper is abundantly reduced, and the yellow precipitate falls to the bottom of the test-tube.

Sugar has, therefore, made its appearance since yesterday in the tissue of the liver; and it is evident that it is a substance, in this instance at least, of local formation.

But you will remember that this sugar has not been directly and immediately formed from the albuminous ingredients of the hepatic tissue. It has been produced by transformation from the glycogenic matter which previously existed in the liver. Now this glycogenic matter is much less soluble in water than the sugar which it produces. It requires a prolonged boiling of the bruised tissue of the organ to extract it. Accordingly, by injecting the vessels of the liver with cold water, immediately after death, all the sugar which it contained is washed out, while the glycogenic matter is left. During the next twenty-four hours, some of this glycogenic matter is converted into sugar by the influence of an animal ferment, also contained in the substance of the organ, and so the sugar again makes its appearance.

This production of sugar, then, in the separated liver, is a phenomenon of *fermentation* or *catalysis*. It requires a certain elevation of temperature for its accomplishment, like all actions of this nature. If the liver had been placed in ice yesterday, immediately after its injection, and kept at a temperature of 32°, or thereabout, no sugar would have been produced in it. The glycogenic matter would have remained unchanged, notwithstanding the presence of the ferment. But at the temperature of 100° F. the conversion of the glycogenic matter into sugar takes place very readily. As fast as the glycogenic matter is formed in the substance of the organ, a part of it is instantly transformed into sugar, by contact with the animal ferment.

The sugar itself is dissolved and taken up by the blood of the capillaries and hepatic veins, as it is by the water, in an artificial injection.

There is a constant production and renovation, therefore, of these two substances in the tissue of the liver. The glycogenic matter disappears by being converted into sugar, and its place is supplied by new material of the same kind, formed by the nutritive processes of the organs. On the other hand, the sugar is absorbed and carried away by the blood, at the same time that a new quantity is produced from the glycogenic matter by fermentation. So that the liver always contains both sugar and glycogenic matter, though both these substances are incessantly renewed and altered by transformation or removal.

Now let us see what finally becomes of the sugar after its entrance into the blood.

We have already found that it is carried by blood of the hepatic veins to the ascending vena cava. There the saccharine blood coming from the liver is mingled with the venous blood returning from the abdomen and lower extremities. It there passes upward and meets the superior vena cava, bringing the blood from the head and upper extremities. Mingled with this new quantity of venous blood, it is then poured into the right auricle and ventricle, and passes, through the pulmonary capillaries, round to the left side of the heart.

Now, during this passage and admixture, the sugar, which had been derived from the liver under ordinary circumstances, disappears, either from the action of some other ingredients of the venous blood, or from some influence of the air, while passing through the pulmonary circulation, the sugar is altered or destroyed in the lungs, so that it cannot be found in the blood of the left side of the heart. We do not know what are the precise details of this transformation, nor into what new substances the sugar is converted; but we know that its transformation is constant, and that as fast as it is produced in the liver it is normally destroyed in the lungs.

Usually, therefore, there is no sugar in the blood of the arteries. In one part of the circulatory system, viz., that between the liver and the lungs, the blood constantly contains sugar; for this substance is produced in one of these organs and destroyed in the other. For the same reason, it is not to be found elsewhere, and there is no saccharine ingredient in the blood of the general circulation.

But Bernard has found that this is only true in the intervals of digestion. For as soon as intestinal digestion becomes active, the secretory function of the liver is increased, a larger quantity of sugar is produced in the tissue of the organ, and a larger quantity is brought to

the right side of the heart by the hepatic veins and vena cava. After a time this increase in the quantity of sugar in the venous blood becomes so great, that all of it is not destroyed in passing through the pulmonary circulation; and the superabundance then passes through into the arterial system, and circulates generally throughout the body.

There is a time, therefore, some hours after digestion has commenced, when the arterial blood everywhere contains sugar, and where this substance passes through the capillaries of all the different organs.

This is not because the sugar is introduced in larger quantity with the food; for the phenomenon will take place after a meal of purely animal food, which contains neither sugar nor glycogenic matter. It is because the increased quantity of blood, returning from the digestive organs, and circulating through the portal system, stimulates the nutritive processes of the liver, and causes an increased formation of its ordinary products. The sugar is produced in greater abundance by the liver during the digestive process, just as carbonic acid is formed in larger quantity by the system generally, at the same time. The nutritive changes go on more rapidly than usual, and the substances which result from their alteration are produced more abundantly.

But as soon as digestion comes to an end, the activity of the hepatic circulation again diminishes, and the production of sugar to a more moderate standard. The superabundance of this substance then begins to disappear from the circulation, by the ordinary mode of its destruction or decomposition. It may be that the decomposition of this superabundant sugar takes place everywhere, by contact with the other ingredients of the blood, as I have already intimated; or it may be that it is accomplished altogether in the lungs, by the blood constantly returning to the pulmonary vessels, in the natural course of the circulation. At all events, after a certain time the sugar has entirely disappeared from the blood of the arterial system, and is again confined to the veins between the liver and the lungs.

This function, of the production of sugar, and its existence in the circulating blood, has a very interesting relation to the disease which we know as *Diabetes Mellitus*.

You remember, gentlemen, the great rapidity with which we found endosmosis and exosmosis to take place in the living body. Any substance, like iodine or ferrocyanide of potassium, injected into the blood, passes out by exosmosis through the mucous membranes and the glandular surfaces. It requires a certain quantity of such a substance injected into the blood, for it to be detected in the secreted or excreted fluids; but different substances require to be injected in different

quantities, and the same substance will appear with different degrees of facility in different organs.

Now sugar is one of those substances which are naturally destroyed in the blood, and which do not, therefore, appear in the secretions, unless introduced in considerable quantity. In the natural process of digestion, although sugar finds its way into the arterial blood, it does not exist there at any time in sufficient quantity to pass out by any of the secreting surfaces.

But in diabetes, the blood contains sugar in unusually large quantity. This substance, then, exists in the circulating fluid, not only during digestion, but at all times. Its quantity is so great that its destruction does not keep pace with its formation, and it therefore circulates constantly all over the body, and is to be found throughout the vascular system. When a certain point of accumulation is thus reached, the sugar begins to pass out of the blood-vessels by exosmosis; and as it transudes most readily by the kidneys, it then makes its appearance in the urine.

It is in this way that the saccharine condition of the urine is produced, which is the most marked and palpable symptom of this disease.

As to the cause of this accumulation of sugar in the blood, in diabetes, it may be accounted for in two different ways: First, the production of sugar by the liver may be more rapid than in health, the rate of its decomposition and destruction remaining the same; or second, the production of the sugar going on as usual, its decomposition may be interfered with in some manner, thus causing its accumulation in the blood. In the first instance, the sugar discharged by the kidneys would be the surplus saccharine matter, produced by the abnormal activity of the liver; in the second case, the sugar, naturally formed in the liver, would be simply discharged with the urine, instead of being destroyed in the interior of the body.

We are entirely ignorant which of these hypotheses is the true one. M. Bernard has performed various experiments which lead him to the conclusion that it is the abnormal activity of the liver which causes the diseased condition, but I do not think they are sufficiently conclusive to warrant a decision on this point. We only know positively that, in a state of health, there is a certain balance between the production of sugar in the liver and its metamorphosis in the blood; and that in diabetes this balance is disturbed, either by an increase in the activity of one process, or a diminution in the other. At all events,

the superabundance of saccharine matter escapes, by exosmosis, through the substance of the kidneys.

But the glycogenic function of the liver has another relation, of a physiological nature, still more interesting and important to us than its connection with diabetes. It leads us directly to the study of the internal metamorphosis, or *renovation of the blood*, which is one of the most remarkable of all the physiological phenomena of the circulation.

Let us see, for example, what is the quantity of the material produced and destroyed, from this source alone, during twenty-four hours.

Lehmann has found, by examining the blood of the hepatic vein, in the dog, that this blood contains, as an average from six observations, about eight parts of sugar per thousand of the dried residue. This makes a little over 1.5 parts per thousand of the undried blood. Now, from the most moderate estimates, it appears that the entire quantity of blood passing through the liver daily, in the human subject, is not less than 69.02 pounds; and the quantity of sugar in the healthy human liver is found, by examination, to be precisely, or very nearly, the same as in that of the dog. The whole amount of sugar, therefore, produced daily in the human liver, carried away from it by the hepatic blood, and destroyed in the general circulation, is at least 792 grains, or a little over one ounce and a half.

This quantity of material, therefore, is transformed and renewed, by a single process alone, in the course of twenty-four hours.

But there is another substance, beside, that is produced and discharged daily in nearly as large quantity as the sugar, viz., urea. This substance has been long known as the most abundant ingredient of the urine, and the average quantity discharged during twenty-four hours by an adult man, according to the investigations of Lehmann, Hammond, and Draper, is about 500 grains. Now, this urea is not produced in the kidneys, but is formed generally throughout the body. At least it first makes its appearance, so far as we know, in the blood of the general circulation. Its proportion, however, in the blood, at any one time, is very minute; so much so, that it can only be detected by using several pounds of blood for analysis, and is then recognized principally by the microscopic measurement of its crystals.

The proportion of urea usually existing in healthy blood has been found, according to Milne Edwards, to be 0.016 per cent., or a little over 1½ per ten thousand parts. Estimating the whole amount of blood in the body at from seventeen to twenty pounds, the entire quantity of urea in the blood at any one time is not over 22½ grains; and yet we know that no less than 500 grains of this substance is

produced in the body during twenty-four hours, since this quantity is actually discharged every day with the urine.

The reason, why the quantity of urea contained in the blood is kept down to so low a standard, is that it is constantly drained away from the vessels of the kidneys.

As the blood is passing through the renal circulation, the urea which it contains filters away by exosmosis, and appears in the urine. As successive portions of blood, also, rapidly pass through the kidneys, one after the other, the entire quantity of urea in the body is reduced to a minimum; for as fast as it is produced in the general system, it is exuded from the local circulation of the kidneys, so that its production and discharge counterbalance each other, and there is but a small quantity left behind in the blood.

Another very curious instance, of a similar nature, is furnished by the history of the *fibrin* of the blood, and its property of coagulation.

We know that the blood naturally contains, beside albumen, another animal substance, termed fibrin, in the proportion of about 2.5 parts per thousand. The distinguishing peculiarity of fibrin is its property of "spontaneous coagulation." As soon as it is withdrawn from the vessels, it coagulates, or solidifies, in the course of a very few minutes, and cannot afterward be made to resume its original form. And yet, during the circulation of the blood in the vessels, the fibrin is always fluid. It only coagulates when withdrawn from the vessels, or when the circulation is interfered with in some way, either by laceration of the vessels, by ligature, compression, aneurism, or other similar means. But under these circumstances the fibrin always coagulates, in a length of time varying from a few minutes to several hours.

Now, it has been found very difficult to explain this property belonging to the fibrin of the blood. If the fibrin have a natural tendency to coagulate, why should it remain fluid in the vessels? If not, why should it coagulate when withdrawn from them, or even within them, when its movement is arrested by a ligature?

Different observers, accordingly, have endeavored to solve this question in two different ways. Some have asked, "What is the cause of the coagulation of the blood?"—others, "What is the cause of its fluidity in the vessels?" Robin and Verdeil, for instance, come to the conclusion that the blood is only kept fluid by the constant movement of the circulation, and that it accordingly solidifies whenever this movement is arrested or retarded, either inside or outside the vessels. Dr. Richardson, on the other hand, wrote a memoir a few years ago, which received the Astley Cooper Prize for 1856, on "The Cause of the

Coagulation of the Blood,"—in which he considers the fibrin as held in solution, in the blood, by ammonia, and regards its coagulation as a kind of precipitation, caused by the escape of its ammoniacal solvent.

But there are several facts which are opposed to Dr. Richardson's hypothesis, and his conclusions have not been generally adopted. The most commonly received opinion for several years has been that of Robin and Verdeil, which regards the fibrin as having a normal tendency to coagulation, owing to its physiological nature and constitution; but as prevented from coagulating by the peculiar movement which is kept up by the circulating blood, in the vessels of the living body. This hypothesis did not attempt to explain how it were possible for such a mechanical movement to prevent the chemical or physico-chemical process of coagulation; it only pointed out the fact that the blood remains fluid while moving through the vessels, and that it coagulates under every other condition.

But it now appears that the fibrin of the blood, like the sugar of the liver, is constantly undergoing a natural metamorphosis and renovation in the interior of the body.

Simon, Bernard, Lehmann, and Brown-Séquard have made a variety of observations, which show that the venous blood, coming from the kidneys and the liver, is either destitute of fibrin, or contains this substance in much smaller proportion than arterial blood. The fibrin, therefore, disappears from the blood, while passing through these organs; and as it still continues to be found in the general mass of the blood in its ordinary proportion, viz., 2.5 parts per thousand, it is evident that it must be as constantly *produced* somewhere in the body, so that its decomposition in the liver and kidneys is counterbalanced. The entire quantity of blood flowing through both the kidneys and liver per day, in the human subject, is not less than 78.5 pounds, or 550,000 grains. The whole amount of fibrin, accordingly, which is destroyed in these organs, and reproduced elsewhere, per day, is 2.5 parts per thousand of the above quantity, viz., 1,375 grains.

When we remember that a pound of ordinary blood contains only 17.5 grains of fibrin, and that the whole amount of this substance contained in the body, at any one time, is certainly not over 355 grains, it is evident that the fibrin of the entire blood is destroyed and reproduced in the circulation, at least three times over in the course of a single day.

We understand, accordingly, why the fibrin of the blood should have the property of spontaneous coagulation, and yet should not coagulate, so long as its circulation is kept up in the vessels of the

living body. The fibrin is not a permanent, but a temporary ingredient of the blood. Each portion of it, when formed by the processes of nutrition, is destined within a few hours to be decomposed or transformed, by the kidneys and liver, into some new substance, of different properties. Before the time has arrived for its coagulation, therefore, it has already undergone this transformation, and its place in the blood is taken by a new quantity, of recent formation.

But if, from any cause, the natural transformation of the fibrin be interfered with, it inevitably coagulates. Thus, if the blood be withdrawn from the vessels, it solidifies in a short time, because its fibrin remains undecomposed. The same effect follows if the blood be extravasated into the areolar tissue, or into the parenchyma of the internal organs; or if it be arrested in the vessels, by the application of a ligature. Even if the movement of the blood be simply retarded, by a varicose condition of the veins, or by the aneurismal dilatation of an artery, coagulation is very apt to take place in that part of the vessel in which the stream is most sluggish.

In all cases, interference with the circulation gives rise to coagulation of the fibrin, in a condition of health.

For the same reason, a local coagulation takes place wherever an obstruction of any kind is met with in the circulatory system. Thus we find fibrinous coagula deposited upon the edges of the aortic valves, when they are thickened and roughened by disease; and during the last hours of life, when the circulation is performed slowly, the blood often coagulates upon the valves and tendinous cords in the ventricles of the heart.

We see, then, that in the liver, the kidneys, and the general circulation, the constitution of the blood is incessantly undergoing alteration, by the simultaneous formation and disappearance of sugar, urea, and fibrin. Another change, of at least equal importance, is that connected with the process of respiration.

We know that the tissues require for their nourishment a constant supply of oxygen. This oxygen is conveyed to them by the arterial blood, which contains it in large quantity, in solution in the blood-globules. While passing through the capillaries of the general circulation, the blood therefore gives up the greater part of its oxygen, and absorbs from the tissues carbonic acid instead. Consequently, it is incessantly altered, from a fluid rich in oxygen and poor in carbonic acid, to one poor in oxygen and rich in carbonic acid.

But in the pulmonary circulation, exactly the opposite change takes place. Here the blood exhales its carbonic acid, and absorbs oxygen

from the atmospheric air in the lungs. Or, more properly, these gases pass, by endosmosis and exosmosis, into and from the blood, through the medium of the pulmonary membrane. The blood, accordingly, during every twenty-five seconds, is undergoing two corresponding alterations, by a double interchange in the proportions of its oxygen and carbonic acid.

Now let us see what are the quantities of these gaseous ingredients which are so frequently renovated.

The ordinary quantity of air taken into the lungs with each inspiration is about twenty cubic inches. Counting eighteen respirations to the minute, this makes 360 cubic inches of air per minute, 21,600 cubic inches per hour, and 518,400 cubic inches per day. But since the movements of respiration are somewhat hastened by any active exertion, the actual daily quantity of air used in respiration is not less than 600,000 cubic inches, or about 350 cubic feet. In respiration, the air loses, upon an average, 5 per cent. of its volume of oxygen. The total quantity of oxygen, therefore, absorbed by an adult man, during twenty-four hours, is seventeen and a half cubic feet, or about four times the bulk of his own body. This equals, in weight, 7,134 grains, or a little over one pound, avoirdupois.

The quantity of carbonic acid exhaled may be ascertained in a similar way. The expired air usually contains about four per cent. of its volume of carbonic acid. According to the results obtained by the best German and French observers, the quantity of this gas exhaled is very nearly 1,150 cubic inches per hour, or fifteen and a half cubic feet per day. This is, by weight, 10,740 grains, or a little over one pound and a half.

Every day, therefore, the blood receives a pound of one material, and discharges a pound and a half of another, by respiration alone.

Beside this, however, a certain amount of aqueous vapor is always discharged with the breath; more abundantly in a dry atmosphere, less so in a moist one.

The blood is also replenished by the absorption, each day, of a large quantity of solids and fluids, derived from the digestion of the food. These substances are taken up by the blood of the portal vein, and, after passing through the liver and the lungs, are mingled with the general mass of the circulating fluid. The average amount of these substances is easily ascertained by experiment.

A corresponding quantity of material is discharged daily by the

skin, the kidneys, and the other excretory passages. The urine, per day, contains a little over two pounds of water, 500 grains of urea, and a smaller quantity of other solid matters. Nearly two pounds of fluid, according to Lavoisier and Seguin, are also discharged with the cutaneous perspiration; and a certain quantity of insoluble material is secreted by the large intestine and discharged with the fæces. From these data I have constructed the following table, showing, in an approximate manner, the total amount of ingesta and egesta for twenty-four hours, as follows:

Absorbed during twenty-four hours.		*Discharged during twenty-four hours.*	
Oxygen	1.019 lbs.	Carbonic Acid	1.535 lbs.
Water	4.275 "	Aqueous Vapor	0.445 "
Albuminous Matter	.340 "	Perspiration	1.965 "
Starch	.590 "	Water of the Urine	2.020 "
Fat	.220 "	Urea and Salts	.150 "
Salts	.056 "	Fæces	.385 "
	6.500 "		6.500 "

No less than six pounds and a half, therefore, are absorbed and discharged daily by the body of a healthy adult; and for a man weighing 140 pounds, a quantity of material, equal to the weight of the whole body, passes through the system in the space of twenty-two days.

We must remember, furthermore, that all this absorption and discharge takes place through the blood. These materials do not simply pass through the body as foreign substances. They are absorbed by the vessels, and become ingredients of the circulating fluid; and are then, in great measure, decomposed in the blood, and finally discharged under other forms. It is the blood which absorbs fat and albuminous matter in one organ, and discharges urea or carbonic acid in another.

But we have already seen that these changes of the ingesta and egesta are not the only ones suffered by the blood in the circulation. There are many substances, formed in the internal organs, which enter the blood at one part of the circulatory system, and are decomposed at another. Liver-sugar and fibrin are both instances of this. Neither of them are introduced with the food, and neither of them appear in the excretions; for both their origin and their decomposition take place within the limits of the organism itself. There is a series of internal as well as external changes, therefore, by which the blood is incessantly altered in constitution as it passes through the ound of the circulation. Beside this, many of the secretions, after

being discharged from one glandular organ, are reabsorbed, and again enter the blood in another part of the body. Thus the saliva, secreted by the salivary glands, is discharged into the mouth, mingled with the food in mastication, swallowed into the stomach, and afterward reabsorbed. The gastric juice, after being secreted by the mucous membrane of the stomach, is again taken up by that of the intestinal canal. The pancreatic juice, which emulsions the fatty substances of the food, afterward forms a part of the chyle, and so enters the lacteals and the portal vessels. Various investigations have now shown that the bile itself, after being produced and discharged by the liver, is reabsorbed from the intestine, and again mingles, under a different form, with the other ingredients of the blood.

Let us now form an estimate of the entire amount of this internal circulation of fluids, thus passing from and returning into the vascular system.

But first, it would be desirable to ascertain the entire quantity of the blood itself. This has always been found a difficult matter, owing to the readiness with which absorption takes place between the blood-vessels and the tissues. For as soon as we begin to bleed an animal, the blood withdrawn by hæmorrhage is partially replaced by a serous fluid absorbed from the tissues; so that while the *volume* of the blood is partly replenished, its *density and constitution* are at the same time altered.

Becquerel and Rodier found that this change in the composition of the blood, during hæmorrhage, took place with unexpected rapidity. The alteration, according to them, is a continuous and progressive one, "beginning," as they say, "with the first drop of blood which escapes, and ending only with the last." They found that, in the human subject, if only twelve ounces were drawn from the arm at one time, there was a perceptible difference in constitution between the blood drawn toward the end of the venesection, and that which escaped at the beginning. In some instances, the proportion of solid matters had fallen from 167 to 163 parts per thousand, and that of the fibrin from 2.42 to 2.17 per thousand.

It is impossible, therefore, to ascertain the exact quantity of blood by a simple bleeding; or even, as it has been suggested, by bleeding to a small quantity, then injecting with a saline solution, and afterward, by a second bleeding, determining the change thus produced in the constitution of the blood. For the blood, as we have already seen, will be altered in its constitution by the bleeding as well as by the injection, and it would not be possible to decide how much alteration was due to either cause.

The best plan yet proposed is that of Lehmann and Weber. They collected the blood of two criminals, who were executed by decapitation, and measured the quantity thus escaping from the vessels. They then injected the vessels of the head and trunk with distilled water, and after collecting the bloody fluid which escaped from the veins, ascertained the quantity of solid material which it held in solution, and from that estimated the quantity of blood remaining in the vessels after decapitation. This, added to that which escaped from the vessels at first, gave a total quantity of from seventeen to eighteen pounds.

This method itself is not entirely accurate. For in a watery injection of the vessels after death, the water not only transudes and infiltrates the surrounding tissues, but it is also liable to absorb various substances from the tissues, in different proportions from those in which they would be taken up by the blood. Still, it is a better mode than any other which is practicable, and sufficiently exact for ordinary purposes. There is no doubt that the natural quantity of blood in the healthy adult man, of average size, is not less than seventeen, and not more than twenty pounds.

But an equally interesting question, in regard to the internal alteration and metamorphosis of the blood, is the rapidity with which the blood passes through the different organs.

We have already seen, in a previous Lecture, that the blood performs the entire round of the double circulation in from 25 to 30 seconds. That is, a certain portion of blood, after passing the middle of the jugular vein, will return to the same point in about half a minute. But how often, during a given time, does the *entire quantity* of blood pass a given point in the circulatory system? In other words, how rapidly does the whole of the blood pass through the whole of the circulation?

This may be ascertained by determining the quantity of blood discharged from the left ventricle at each pulsation of the heart. This quantity is usually estimated at two fluid ounces, and it is by some taken for granted that the ventricle empties itself completely of blood at each pulsation. This conclusion has been drawn from the appearance of the left ventricle when contracted, as it usually appears some hours after death. But this *post-mortem* contraction, as I have already told you, does not represent the natural condition of the ventricle during life, but is a true cadaveric rigidity, like that of other parts of the muscular system.

But even in the condition of *post-mortem* contraction I have not

found the cavity of the left ventricle so completely obliterated as we might be led to suppose. In making transverse sections of the left ventricle of the bullock's heart, in this condition, at different levels, it can be seen that, while the opposite surfaces of the ventricle are in contact with each other toward the apex of the organ, they are always separated, by a certain space, at its middle and toward its base.

I have also measured the capacity of the left ventricle of the bullock's heart, after cadaveric rigidity was fully established, by filling it with water, up to the level of the mitral and aortic valves, with the following results:

Bullock's heart	No.	1,				contained	$1\frac{3}{4}$	fluid ounces.
"	"	2,				"	2	"
"	"	3,	(weighing	4	lbs.,)	"	$2\frac{1}{4}$	"
"	"	4,	"	$4\frac{3}{4}$	"	"	$1\frac{7}{8}$	"
"	"	5,	"	5	"	"	$1\frac{1}{2}$	"

It has always appeared to me, beside, in watching the movements of the heart, when exposed in the living animal, that the ventricle does not empty itself completely at each pulsation, but that, like the arteries, it always contains a certain amount of blood, when contracted as well as when in a state of dilatation.

If we estimate, therefore, the quantity of blood discharged at each ventricular contraction at half an ounce, and the number of cardiac pulsations at seventy per minute, this will give 35 ounces of blood discharged per minute, 2,100 ounces, or about 130 pounds, per hour, and 3,120 pounds per day. According to this calculation, the entire quantity of blood in the body passes through the heart at least 150 times over, in the course of twenty-four hours.

I have endeavored to ascertain the quantity of blood thrown out rom the heart in a given time, by direct experiment. For this purpose, I produced insensibility in a dog, weighing fourteen and a half pounds, by the inoculation of woorara, keeping up artificial respiration by means of a bellows inserted into the trachea; then opened the chest, and exposed the heart and arch of the aorta. On cutting across the aorta, just above its origin from the left ventricle, I found hat 300 grains of blood escaped from the divided orifice in fifteen seconds. This would give 1,200 grains per minute, 72,000 grains per hour, and 247 pounds per day, or about 17 times the weight of the whole body.

This experiment, even, cannot be regarded as accurate. For, on the one hand, when we cut across the aorta, the blood is relieved r om the resistance of the capillary system, and therefore has a ten-

dency to flow out of the wound more rapidly than it would pass through the vessel during life; and on the other, the opening of the chest and exposure of the thoracic organs diminish very much, as we have already seen, the force of the heart's action, and therefore lessen the quantity of blood which would be discharged from it. However, if we apply this result to an adult man, of 140 pounds weight, we find that the entire quantity of blood so discharged would be 2,380 pounds.

It is safe, therefore, to regard this estimate as below rather than above the truth; and there can be no doubt that, in the human subject, the quantity of blood passing daily through the heart is considerably over two thousand pounds.

A proportionably large quantity, of course, passes through all the vascular organs in the same length of time.

Now let us see in what quantities the various secretions are produced from the circulating fluid.

Without going over all the details by which these quantities have been ascertained, I will merely mention that in each case the estimates have been arrived at by direct experiment, so that nothing is left to surmise; and that the numbers given are always the minima, rather than the maxima, of those adopted by the best authorities.

The total quantity of *saliva* secreted daily is about 20,164 grains, or a little less than three pounds avoirdupois. The quantity of *gastric juice* is much greater. The most moderate estimate, based upon actual observation, gives fourteen pounds as the total daily amount; and from my own experiments, I have no doubt that this is substantially correct. The *pancreatic juice*, again, seems to be considerably less in amount—not much over one-third of a pound in twenty-four hours. From the very careful and judicious observations of Bidder and Schmidt, the daily quantity of *bile* is found to be not far from two and a half pounds. The quantity of the *intestinal juice* has never been ascertained with any approach to accuracy, though it is no doubt quite large, since the follicles of Lieberkühn are very abundant, and the extent of secreting surface in the intestine not less than from seven to eight square feet.

At least from nineteen to twenty pounds of fluid, therefore, are separated from the blood by the glandular organs every day.

But we must recollect that these secretions are all reabsorbed in the natural course of the circulation, and again enter the vascular system at different points. The blood, accordingly, is not impoverished by this abundant supply of secreted fluids, but only renovated

and altered; for the quantity of material withdrawn from it by one process is constantly returned to it by another.

You can estimate, therefore, gentlemen, the incessant changes which take place in the circulating fluid, during its passage through the vascular system. The relation between the blood and the various organs and tissues is one of mutual dependence and interchange. The blood absorbs from the tissues, and the tissues absorb from the blood; and so rapidly and extensively does this process go on, that a quantity of fluid equal to the whole mass of the blood passes and repasses through the secreting and absorbing surfaces every day. Every organ, too, produces its own peculiar modification in the blood which passes through it. Even the *quantity* of the blood is variously modified in different organs; for in some its mass is increased, in others diminished. In the intestine, for example, the blood absorbs more than it loses; in the kidneys, on the other hand, it is diminished by exhalation, without any corresponding supply.

Beside this, we have already seen that the blood is undergoing a constant internal alteration, by the alternate formation and disappearance of its own ingredients. Some of these are formed in the blood, such as fibrin and urea; others are absorbed from the tissues, such as cholesterin and carbonic acid. Some are decomposed in the circulation itself; others are discharged and eliminated by the excretory organs.

We can understand, then, how complete is the change effected by all these processes in combination. It is not only certain ingredients of the blood which are modified or altered in the circulation; but it would be more proper to say that the *entire blood* is renovated and transformed, while passing through the vascular apparatus. It was an opinion entertained by some of the older physiologists, that the arterial blood was used up and appropriated by the tissues, and that the venous blood was a fluid of new formation, supplied by the tissues themselves. We see now that this opinion was very nearly a true one, though in a different sense from that in which it was at first understood.

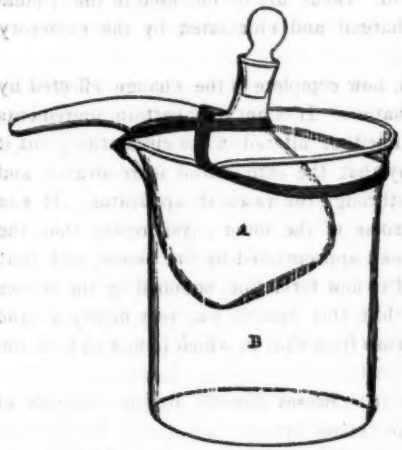
The arterial blood, it is true, passes directly, by the channels of the capillary vessels, into the venous system; but during this passage it is almost entirely changed. The blood-globules lose their oxygen and absorb carbonic acid; and the ingredients of the plasma are taken up by the tissues, transformed, or replaced by other substances. *The blood, therefore, which passes out of an organ by the veins, is not the same blood which passed into it by the arteries.* The ingredients which it has

lost have been decomposed or imbibed by the neighboring tissues; while those which it contains instead, have been absorbed from the substance of the organ, or produced in its interior by the mutual reaction of the solids and fluids.

These, gentlemen, are the regular and incessant alterations which the blood undergoes in the course of the circulation. It remains for us to study the variations which take place, in these processes, at different times, and in different parts of the vascular system.

A New Instrument for the Novel Application of Anæsthetic and Stimulating Vapors for Deafness, Neuralgia, &c. By H. P. Dewees, M.D., New York.

Dr. Douglas: *Dear Sir*—In reply to your note respecting my instrument for etherization and vaporization in certain diseases of the ear, I will furnish you with a drawing of its arrangement, and a description of its use, as employed by me not only in aural disorders, but also in neuralgia requiring local anæsthesia, and some diseases of the spinal column in which topical irritation is called for, varying from an artificial glow up to absolute cautery.



The instrument originally used by me during the last fourteen years is still in my possession; but, from the inconvenience and imperfectness attending vaporization by the heat of the hand, or by holding it in warm water, I have laid it aside, and have contrived the one to which you refer. This instrument (see cut) consists of a delicate Bohemian glass retort, with a nozzle projecting an inch and a half, perforated by a capillary aperture. The supply-tube of the retort rises about an inch above the level of the curved neck, thus allowing greater freedom in pouring in the fluid to be evaporated. A cork stopper, with an elastic cap or holder to prevent its expulsion, or a ground-glass

capillary tube stopper, closes this entrance, the one or the other to be used according to circumstances of application. The retort, when charged with the fluid to be evaporated, is then lodged within a hard glass receiver, three inches high, having its rim lipped deep enough to embrace the projecting nozzle of the retort, thus affording greater steadiness, besides serving as a catch for the elastic band retaining the nozzle in its projection. Small nipple-like catches are studded on the opposing centres of the rim of the receiver, to afford resisting points for the elastic back band which passes from under the posterior rim across the front of the supply-tube. By these two elastic bands, the mobility of action is fully commanded in the projecting nozzle.

The mode of using this simple but astonishingly effective little instrument requires but little practice to regulate it to the necessities of the case. About two drachms of ether are to be poured into the retort when secured in the receiver. The stopper should be firmly fastened, and the finger can be readily placed over the nozzle aperture during or after the introduction of the warm water or sand into the receiver. A rushing or blowing sound of the escaping vapor or gas immediately ensues through the aperture, if uncovered. The nozzle is then introduced within the auricle, and a little experience will soon teach the time and proper distance (from the drum) of the application. Moderately warm water will only be required for sulphuric ether; the degree of heat to be varied according to the specific gravity of the evaporable fluid, and according as a slow or rapid disengagement is needed.

Besides its use in etherization of the ear, this little instrument affords in neuralgia the most elegant mode for the application of local anæsthesia; the constant current from the evaporating ether, chloroform, &c., being readily applied directly to the suffering part, and made to follow the course of the painful nerve. When it is wished to apply the anæsthetic locally, I use a ring of adhesive plaster or of kid, to prevent the diffusion of the vapor over the surrounding parts. A common pill-box, perforated to admit the nozzle of the instrument, and applied over the part, also answers well. In inhalation, or in etherization, through the Eustachian tube, the capillary glass stopper will be found preferable, as it admits a sufficient admixture of air, as well as an outward communication for breathing.

In certain spinal or nervous disorders, this instrument affords a most ready and exclusive mode of applying heat, which can be made to vary from a simple glow to the moxa or absolute cautery. This is effected

by igniting the jet as it passes through the capillary tube of the nozzle, the amount or force being regulated by the evaporizing temperature employed, and the distance from the part. The circular protections above mentioned are useful when the moxa is required.

In some of the diseases of the ear, where, besides the anæsthesia to the inner nervous distributions, a *local stimulant* or *irritant* to the drum or the auricle is wanted, this instrument offers a ready method of effecting the desired end, viz., by adding to the ether, or other fluid, a volatile stimulant, as for instance, the Ess. ol. sinapis, diluted. No practitioner should be without one, from the extensive capacity of the varied adaptability of the instrument. In diseases of the ear or in neuralgia, it can safely be intrusted to a patient for self-application. Many volatile substances can be directly applied through its agency.

In closing this note, I will state that Messrs. J. F. Luhme & Co., No. 556 Broadway, are the sole manufacturers of this instrument, and, I believe, at the price of $1.00.

791 Broadway, N. Y., *Sept.* 17, 1860.

MONTHLY SUMMARY OF MEDICAL JOURNALISM.

By O. C. Gibbs, M.D., Frewsburg, N. Y.

Diphtheria.—In the *Lancet and Observer* for August, Dr. C. A. Hartman, of Cleveland, Ohio, has quite a lengthy paper upon the subject of diphtheria. It is a thorough *résumé* of the subject, and the opinions of the most authoritative in the matter are fully stated. Though twelve pages of treatment are given, the author does not give his own opinions upon that all-important part of the subject. We refer to the paper for two purposes: first, to remark that it is the best *résumé* of the subject we have seen, showing that the author has read all that has been written upon the subject, and as embracing the opinions of the various authors in one article; and *second*, to refer to the statement of one complication, which complication we have observed, and yet, we think, is often overlooked. He says, "latent pneumonia is frequently discovered in the dead body." The remark we wish to make here is, that we believe there is a peculiar liability to this complication in diphtheria. We have observed it so often, not in the "dead body," but the living patient, that we cannot think it altogether an accidental circumstance. In full one-half of the cases that have fallen under our observation we have discovered the physical signs of pneumonia. The complication is seldom discoverable except by the physical signs, as the patient complains of no pain about the chest; often there is no cough, and no expectoration, other than might be expected to come from the throat.

In a former paper we have said that we considered whiskey superior to all other medication in this disease. The question which would naturally suggest

itself to the minds of our readers is, should this be withheld in the pneumonic complication? We have not withheld it, and have seen no bad results from its continuance. The only special treatment we give such cases is opium in tolerably full doses in combination with ipecac, and the application of a blister.

In the *Chicago Medical Examiner* for August, the editor, Prof. N. S. Davis, has an article upon diphtheria. He says, "We regard diphtheria as an affection intermediate between scarlatina and membranous croup." In regard to treatment, he recognizes three indications to be fulfilled: "1st, to restore the normal condition of the blood by increasing the solubility and excretion of the fibrin and thereby remove the tendency to further pseudo-membranous deposits in the fauces or elsewhere. 2d, to correct the perverted properties of the solids, and thereby restore a more healthy secretory action generally, and arrest the febrile movement. 3d, to mitigate the local inflammations." In the fulfillment of the first indication he makes use of "the chlorates of potassa and soda," and if need be the muriated tincture of iron. To fulfill the second indication, he resorts to calomel and Dover's powder at first, and subsequently the following:

"R.—Nitrous ether, ℥jss.
Tinc. gelsemin, ℥ss.
Tinc. belladonna, ℥j.

Mix, and give from 10 to 30 drops, according to the age of the patient, every three or four hours." To meet the third indication he resorts to local measures principally, in regard to which there is nothing new, and we have not the space to recapitulate.

In the *Boston Medical and Surgical Journal* for August 16th, Dr. L. R. Morse has also an article upon the subject of diphtheria. He asserts that "the present epidemic, 'Diphtheria,' is neither more nor less than the old 'putrid sore throat,' but now spreading both in Europe and America, in a more decidedly *epidemic form* than usual." In regard to treatment nothing peculiar is suggested. He speaks highly of "strong salt and water" gargle, as we have done before. Even better than this, under some circumstances, he thinks is "the strong hydrochloric acid, diluted with four to eight parts of water," and applied by means of a sponge. The general treatment, he thinks, should be much the same as in typhoid fever. In regard to stimulants he says, "I feel bound to object, in the most decided terms, to the indiscriminate use of the *very large* quantities of brandy often resorted to in this disease." Upon the subject of whiskey in this disease, our opinions have been already expressed.

In the *American Medical Times*, for August 11th and 18th, Dr. A. Jacobi has an able article upon the subject of diphtheria. Our limited space will not justify a synopsis of his opinions, but we shall try to find space for a remark or two in regard to treatment. In regard to local measures, Dr. Jacobi recommends the application of a solution of nitrate of silver, tannin, hydrochloric acid, &c. In regard to general treatment, he says, "Mercury must be avoided; no blood be drawn; no vesicatories applied; in a word, no antiphlogistic treatment should be resorted to." * * * "Emetics must be avoided as much as possible; but they will sometimes prove necessary to remove accumulations of mucus or macerated membrane." Muriated tincture of iron, chlorate of potash, and nitro-muriatic acid, are each well spoken of. Dr. Jacobi's remarks upon quinine are worthy of being quoted entire. He says, "Of the greatest value is that powerful febrifuge, quinine. We have but seldom

used it as a tonic, but generally in one or two large doses (from five to ten grains in children, of the sulphate or muriate, the latter containing more quinine) daily. We have never seen any bad effects, but have always found a great and rapid remission of the fever. If one dose was taken, we ordered it in the afternoon, usually between three and five o'clock; another was sometimes taken in the morning for one or two days, until there was no necessity of administering it twice. A *conditio sine qua non* is a full dose. A child of a year or two must not have less than five grains in a daily dose; we have even to children of two or three years repeated doses of ten grains, and have been fully satisfied with its effect. We have in no disease observed less cerebral symptoms attributable to the effect of quinine than in diphtheria." Albuminuria is a frequent symptom in diphtheria; of this symptom, Dr. Jacobi says, "Albuminuria requires tannic acid; we seldom give any other remedy, and warmly recommend it." Dr. Jacobi's experience in this disease is probably equal to that of any man's in America, and yet he says, "We have no particular remedy to recommend; the treatment should vary according to the case."

Linear Extraction of Cataract.—In the *N. O. Medical News and Hospital Gazette* for August, Prof. C. Beard has an able article upon operations of the eye, with the report of twelve interesting cases, accompanied with eighteen lithographic illustrations. We will not attempt a *summary* of the cases, but will quote the conclusion of one case, in which four operations (two on each eye) were performed, and an accompanying remark, containing an important practical suggestion. The two operations upon the first eye were successful. The first operation upon the second eye gave the following results: "The cornea and conjunctiva natural; iris pressed forward into the anterior chamber, presenting considerable convexity, but no discoloration or other marks of disease; pupil, although dilated, somewhat irregular, but, as far as could be seen, not adherent."

"The case was abandoned to absorption for a few weeks, at the end of which no perceptible change had taken place in the condition of things. It may not be improper to remark, in this connection, that as far as my experience extends, absorption is exceedingly slow in cases presenting the condition which resulted from this operation." * * * "I therefore determined, in this case, to strike at its foundation, and remove the unabsorbed lens by linear extraction; this was performed in the following manner: The patient being in a semi-reclining position, a Beer's cataract-knife was passed perpendicularly to the plane of the cornea, at a point corresponding to the outer margin of the dilated pupil, the point of the knife being pressed sufficiently deep to make an incision two lines in length. The curette was then introduced through the incision, the convex portion resting on its outer lip; very slight pressure upon the globe sufficed to force out the ruptured lens, nearly the whole being removed. A large space of black pupil was the result. Care was taken that no particle of the cataract should remain between the lips of the wound on withdrawal of the curette. The patient's eye was closed with isinglass plaster, iced applications were made, and atropine used for three days, during which time no inflammatory symptoms occurred. Upon opening the eye, union, without perceptible cicatrix, was perfect; patient's vision good. Since that time, the pupil having contracted in such a manner as to conceal the ragged periphery of the capsule, the organ presents a perfectly natural appearance. Sight as good as in the left eye.

"The chief point of interest in this case, as far as regards the right eye, is the rapidity with which a cure was effected, under circumstances which, if the needle alone had been used, would have required months for its performance, to say nothing of the dangers incurred from second, third, or even fourth operations, which are sometimes necessary before the complete removal of the soft cataract by absorption.

"I think it to be a settled question that wounds of the cornea, whether incised or punctured, heal more readily, and give rise to less disturbance, either functional or organic, of the globe than those of the sclerotic, and am resolved hereafter, in all cases of soft cataract in which absorption does not very rapidly take place, after a first operation for solution, to remove the disorganized lens by the method practiced in this case. If I am thought hasty in this conclusion, as to the value of linear extraction under such circumstances, I can cite four or five instances in which I have, by this method, successfully removed soft cataracts of traumatic origin."

Spina Bifida.—We have previously referred to Prof. Daniel Brainard's plan of treating spina bifida with injections of iodine. In the *Chicago Medical Journal* for August, Prof. Brainard reports another case (his sixth) thus treated. The patient was eight months old, and the tumor "measured six inches in circumference around the base, eight inches around its largest part, and was elevated two inches above the surrounding skin." Two injections were made, with an interval of eight days. "During the operation a tape was tied tightly around the base of the tumor, so as to cut off the connection with the spinal canal, and the child kept under the influence of chloroform." Pressure was made after the injections, and thirty days after the first injection the tumor formed "only an innodular mass, diminishing in size." Prof. Brainard says, "In any similar case I should desire: 1st, to tap the sac, and draw off the serum; 2d, to make compression, so as to prevent the iodine from entering the spinal canal; 3d, inject a solution of iodine, of the strength of five grains, and thrice that quantity of iodide of potash to the fluid ounce of distilled water; 4th withdraw the injection, wash out the sac with distilled water. The puncture should be carefully closed after withdrawing the canula."

Before the *New York Pathological Society*, Dr. W. Detmold (*American Medical Times*, August 11th,) reported a case of *spina bifida*, effectually treated by repeated punctures. He remarked that this was the only case he had seen get well under any operation. In the discussion before the Society, which followed the report of this case, it is a little surprising that the treatment by injections of iodine, first proposed by Velpeau, and subsequently repeatedly practiced by Prof. Brainard, of Chicago, with comparative results far more successful than has followed any other operation, should not have been referred to.

Cephalic Version.—In the *Kansas City Medical and Surgical Review* for July, Dr. C. A. Logan has an article upon the above subject, with the report of a successful case. The case was one of shoulder presentation, and we give Dr. Logan's treatment: "As the woman would lie in no other position than on her back, having been previously confined so, I placed her crosswise in bed, near its edge. The hand of the nurse was then placed on the abdomen over the region of the pelvic extremity, and directions given to make pressure upward and inward towards the median line. My left hand was applied over the child's

head, with the intention of pushing it towards the strait at the proper time. The right was gently introduced, and three fingers placed upon the shoulder, and firm pressure made in the absence of a pain, obliquely from left to right, and from below upward. After some little time thus spent, being obliged to desist during a pain that ensued, I had the satisfaction of feeling the shoulder recede before my pressure, and in a hurried effort to grasp the head, as might have been expected, I broke the membranes. A violent pain immediately came on, but I succeeded in fixing the head in the superior strait in the second position of Churchill, (the *right* shoulder had presented,) and retaining it there until it was forced down into the excavation.

The child was born in forty minutes after this procedure, and although small, was very vigorous. This operation, although crowned with a success that surprised me, I am not disposed to go off into ecstasies over, and declare that it is *the superior* of all others, and that it *alone* should be practiced. On the contrary, I am satisfied that, had the membranes been ruptured for any length of time, so as to permit the uterus to contract firmly upon the body of the child, and wedge it down into the cavity, that no amount of force that could be applied consistent with the integrity of the soft parts of the mother, could have dislodged it."

This subject is one of interest, for statistics show that one-third of all the children that are delivered by turning are lost, and one in sixteen of the mothers, in which turning by the feet is performed. If cephalic version is adapted to a certain portion of mal-presentations, and is safer to mother and child, it should be generally known and practiced. In the MONTHLY for August, 1856, Dr. H. G. Carey has an article upon this subject, from which we propose to quote a remark or two. He says, "Few propositions are better established than the complete control which the accoucheur has over the child, prior to the rupture of the membranes."—(A. M. MONTHLY, vol. 6th, p. 89.) He says further, that he is of the opinion "that the almost total exclusion of cephalic version from the lying-in chamber for two and a quarter centuries has resulted in the loss to society of many a valuable life that otherwise might have been saved."—(A. M. MONTHLY, vol. 6, p. 90.) From page 95 of the above-mentioned volume of the MONTHLY we quote the following from Dr. Carey's paper: "*Cephalic version* is applicable to all those cases of deviations of the vertex, in which a reasonable hope is entertained of spontaneous delivery taking place, safely to the mother, if the head were the permanent presenting part. In transverse positions of the child, when the vertex is nearer the superior strait than the pelvic extremity, I think the operation commends itself to the careful consideration of the practitioner. It consists in carrying the presenting part—shoulders, arm, chest, or any other portion of the fœtus that may offer—upward and in an opposite direction to that at which the head of the child may be situated, and as the hand is withdrawn it is made to grasp the vertex, and place it in the most favorable position for delivery." Dr. Carey says further, "The advantages of cephalic over podalic version are simple and readily expressed. *It is safer to the child.* All the circumstances which complicate and render podalic version dangerous to the infant, are avoided by bringing back the head to its normal position at the superior strait. Violence to its person and shock to the nervous system of the fœtus are obviated. So, too, of the fatal compression of the umbilical cord by the detained head in the pelvic cavity of the mother. Security

to life should be paramount to all other considerations with the physician; and if cephalic version can be performed, the above consideration alone should nerve his arm, and inspire him with energy and perseverance to accomplish the delivery of the fœtus, in these cases, by the head." * * * "If the accoucheur has the power of *selecting his time* for changing transverse positions of the fœtus in utero, there certainly is not the least excuse for him, in not placing the vertex *in any position* which his inclinations or the necessities of the case demand." * * * "So long as the child is invested with the membranes containing the amniotic fluid, so long is it under the most perfect control of the skillful manipulator. This axiom requires no arguments for its demonstration. If, then, in this condition, a transverse position of the fœtus is discovered, and the passages are lubricated with the natural secretion, and dilated or dilatable, the practitioner is highly reprehensible in not attempting, at least, to restore the head to its original position. The barriers to his success are trivial, and when he has accomplished his purpose he guarantees to the child one additional chance out of every three for living, over version by the feet." We should be glad to make large quotations, but want of space forbids. We make but one more: "With a limited experience, I perhaps should not presume to call in question the propriety of an operation so generally condemned as is cephalic version; and yet it does appear to me that the objections by which it has been set aside are neither sustained by sound argument nor rational philosophy. All admit the superiority of the operation, and especially the security which it confers upon the child, when judiciously performed; and still most authors exclude it from the safe operations in obstetrics, on the score of *impracticability*. In my humble judgment, in the early stages of shoulder presentation, or transverse positions of the child, when the upper half of the body presents, if the membranes are still intact, or recently ruptured, and the uterus is pliant and yielding under gentle pressure, cephalic version is eminently practicable, and less hazardous to the life and well-being of the infant and mother than podalic version. If this be true, it certainly should be attempted in all cases of the above accidents, when circumstances warrant or demand a change in the position of the fœtus."

Thus it will be seen that cephalic version can be performed, and its attempt is advised by many judicious physicians both at home and abroad. For ourselves, we believe that if the physician should be so fortunate as to discover the shoulder presentation or cross-position of the fœtus, a few hours before the commencement of labor, or, if later, and while the uterine contractions were yet but feeble, his attempts at cephalic version would be far more likely to be crowned with success. When the waters have been for some time evacuated, and the uterus has contracted firmly upon the child, its accomplishment is much more difficult. Yet we confess we cannot see in what respect the difficulty or danger is greater, in passing the hand into the uterus, seizing the shoulder, and so lifting as to make the head describe a *third* of an arc, than in passing the hand and *arm* into the uterus, seizing the feet, and bringing down so as to make the head describe *two-thirds* of an arc.

Syphilis a Cause of Scrofula.—In the *Nashville Journal of Medicine and Surgery*, Dr. L. M. Wasson has an able article upon the above subject. The doctrines advocated are not altogether new, having been previously advanced by Vidal and others, yet the paper is none the less interesting. We make but

one quotation. "That syphilis is a cause of scrofula in the offspring, there can be no doubt, when we take into consideration the feeble organization of the new being; that it has in its incipiency been vitalized from an unhealthy semen, the production of unhealthy blood. The vitality and organization of the children of syphilitic parents are so weak and imperfect, that we know many are aborted before the seventh month of uterine gestation. Many others that come to their full term, perish within the first year, while others before puberty. The deglobulization of the blood during the syphilitic diathesis would seem more than probable, from the general appearance of the persons thus affected. In fact, Grassi is said, from analysis, to have shown the globules of the blood of syphilitic patients to be diminished, and the albumen increased. This inequality of the constituent parts of the blood impairs its nutrition, and of course thereby disqualifies it for the building up of a healthy system with a sound constitution. It also disqualifies it for the healthy reproduction of the species." Of our promise to make but one quotation we repent, and make a second. "Should the scrofulous diathesis be set up, I see no reason why it should not be handed down to the offspring from a syphilitic ancestry, as well as from an original scrofulous ancestry, who had never had the syphilitic taint of constitution. It seems to me that it can be, and I think pathologists will bear me out in the assertion, that it matters not what the primary and exciting cause may be, that tuberculosis is as equally transmissible when caused from cold, as when from a stanched atmosphere, confinement, bad diet, or from any other known and acknowledged cause. It is certainly not the exciting cause that makes it transmissible from ancestry to offspring, but the peculiar diathesis or constitution of the system of the ancestry who transmit it. Scrofula can never take hold of a vigorous constitution until it is first, by some means, prostrated, its vitality impaired and brought down to that standard that is well known and acknowledged to be favorable to tuberculosis. Syphilis is everywhere acknowledged to impair and depress the constitution, and to bring about a cachectic condition of the system. Then, in the absence of statistical data to substantiate the fact that syphilis is a most powerful and frequent cause of scrofula, I take it for granted, from the undeniable fact of its giving the system, in every particular, the scrofulous diathesis, that it is a most powerful and frequent cause of scrofula."

Abortion.—In the *Nashville Journal of Medicine and Surgery*, for August, Dr. John H. Morgan has an article upon the causes of, and means for, the production of abortion in the negro population. He believes that abortion is frequently accomplished among the blacks, as well as whites, and yet he believes there is no remedy that, when administered by the mouth, is *sure* to produce this result. He speaks of *tansy, rue, cotton-plant, ergot, camphor*, &c., as being occasionally used for the purpose of producing abortion. He has but little faith in these remedies, and says, "I will not say that it is impossible, but it is certainly very difficult to effect an abortion with medicines alone, in a stout, healthy and vigorous woman, with no local determinations. I do not believe that we have any specifics in medicine for any disease." He says further, "From what I can learn, camphor is employed more than any other agent as an abortive. The question has been put to me often, both by white and black, if camphor would not make a woman miscarry. With what success it is used, I do not know, but from the extent it is employed it must effect something. It

is employed extensively as a preventive of conception; those who have been accustomed to using it, say that they take it just before or after menstruation, in quantities sufficient to produce a little nervousness for two or three days; when it has this effect they consider themselves safe.

"I am cognizant of the fact that a good many women who are not fruitful after the first birth, use camphor freely, and I have frequently detected its use in this way by the effect on the secretions."

Not long ago we were called to a case of abortion; the ovum had been expelled, and the uterine contractions were severe and painful, and the womb was forced down upon the perineum. On inquiry, we ascertained that medicines had been taken for the purpose of procuring abortion—the articles given, it was said, were none other than madder and whiskey. Physicians generally have no confidence in the emmenagogue properties of the madder, yet we must confess that in this case the uterine contractions were strong, and the lady previously had robust health. She was supposed to be about six weeks advanced in the period of gestation, and under full doses of opium and the recumbent posture, she made a good recovery.

Hypnotism.—Hypnotism has not attracted much attention in this country, though our more excitable and visionary French neighbors have been repeatedly urging the subject upon our consideration. In the *Nashville Journal of Medicine and Surgery*, for August, Dr. W. H. Hancock has an article upon the subject, in which he details two experiments. He says, "The subject of my first experiment was a young lady, in good health, aged about seventeen years. She was seated in an easy-chair, and a spectacle-glass placed about ten inches from the eye, in the median line of vision. A candle was then placed in a position so as to be seen in the glass. When all was arranged, I directed her to look solely at the candle reflected in the glass, and in about six or eight minutes I discovered the telling effects of the experiment. Her eyes began to look very red, her countenance remained immovable, her respiration at the same time becoming difficult, with contraction of the ocular and palpebral muscles—a condition resembling catalepsy; the eyes at this time becoming perfectly closed—indeed, she was asleep." During this sleep, the subject was insensible to pain. Dr. Hancock repeated the experiment upon himself with like result.

Metallic Ligatures.—In the *American Medical Times* for August 11th, Dr. John Watson has a clinical lecture on *amputations*, in which the subject of metallic ligatures is incidentally discussed. As the subject of metallic ligatures is attracting a good deal of attention of late, we shall quote a passage or two. "A good deal is said, now-a-days, about silver sutures and silver ligatures, as if it were really a new thing. Here lies on this table a book, written three hundred and twenty-five years ago by John Rhodius, of Padua, on the use of metallic ligatures, published in the year 1639, in which he gives the authority of the ancients, as well as of the surgeons of the middle ages, for the use of these materials."

"The great use, gentlemen, of the metallic ligature is this: not that they are any better than silk, but because you can use a finer piece, and *the finer the suture the less the irritation.* If you use a very fine wire, you have more power than thread. I don't wish to be understood as disapproving of the silver ligature; it is a very good thing in its place; it is, in fact, an admirable thing in some operations, as, for example, in our friend Sims' operation. Its

great use in the treatment of vesico-vaginal fistula consists in the fact, that but exceedingly small holes are made in the mucous membrane, and hence there is no chance for the escape of the urine into the vagina, a circumstance which would be very apt to occur if an ordinary silk suture was made use of. In all such operations, then, the silver wire is by all odds the best; but this is not the case in the ordinary ligation of vessels. I would advise, in all these cases, the silk ligature, or if you prefer it, the common flax thread, although the former possesses more smoothness, is more round, and can be made stronger." Referring to the experiments of Mr. Jones upon the subject of ligatures, he says, "Mr. Jones made a great number of experiments, thirty or forty years ago, on various animals, to try the effects of the different kinds of ligature upon the vessels of each. He has demonstrated, as the result of these experiments, that a small *round silk thread is the true beau-ideal of a ligature*."

MONTHLY SUMMARY OF FOREIGN MEDICAL LITERATURE.

By Dr. L. Elsberg.

I. MEDICAL PHYSICS AND CHEMISTRY.

1. *Means of Determining the Quality of Milk.* By Dr. H. Minchin. (Dublin Medical Press, August 8, 1860.)
2. *On the Adulteration of Cow's Milk and its Detection*—A Review. By Dr. H. Huppert. (Schmidt's Jahrbücher, CVII., pp. 286, September, 1860.)
3. *On the Determination of the Proportion of Solids in the Urine of Health and Disease.* By William Seller, M.D. (Edinburgh Medical Journal, August, 1860.)
4. *On the Method of Detecting the Presence, and Estimating the Quantity of Sugar in Diabetic Urine, by means of Concentrated Sulphuric Acid.* By James McGhie, M.D. (Glasgow Medical Journal, April, 1860.)
5. *The Amount of Water in the White and Gray Substance of the Brain, and its Relation to Cerebral Œdema.* By Marcé. (Schmidt's Jahrbücher, CVII., pp. 154, August, 1860.)
6. *On a New Form of Chloride of Sodium.* By M. Tusan. (Dublin Medical Press, August 8, 1860.)
7. *Substitute for Gutta Percha.* By Dr. Kirr. (Annales Méd. de la Flanare Occident, 1860.)

1. Reviewing the different modes of milk-testing suggested, aside from chemical analysis, (the principal being by means of, 1, the lactometer, or cream-test, of Sir Joseph Banks; 2, the hydrometer, or specific gravity test; 3, the lactoscope of M. Donné; and 4, the microscope,) Dr. Minchin shows that "an instrument which, in the hands of ordinary observers, will supply the means of determining approximately, or in a rough way, without much trouble, and in a short time, the comparative richness of milk, is still a desideratum." He concludes, "Many persons are able to judge pretty accurately as to the quality of milk,

by carefully observing the transparency which the fluid exhibited when poured in a thin film from one vessel to another; and it would appear that this property, which has already suggested the instrument of M. Donné, might be again turned to account in the construction of a more simple instrument, which would indicate definitely, and enable us to register numerically, the degree of transparency possessed by a given sample; and we should be thus in possession of a very efficient means of estimating the degree to which the milk had been diluted, or how far it fell short of the average quality.

"Such an instrument has lately been invented; the principle of its construction is extremely simple, and the experiments instituted with a view of testing its performance, several series of which have been repeated, appear to have been attended with the most satisfactory and encouraging results. The instrument is made of brass, in the form of a shallow, oblong vessel, capable of containing about an ounce of fluid; the depth of the vessel is made to increase gradually, by means of a slab of white enamel fixed in a gentle slope from one end to the other; this slab is graduated throughout its entire length. Upon this the milk is poured till the vessel is filled, and a cover of plate glass is then put on—this should be done by giving it a sliding motion, to exclude air-bubbles. When the vessel full of milk is thus covered, the degree of dilution possessed by the sample under examination is estimated by the number of degrees on the enamel which can be read through the glass cover; for the glass being in contact with the edge of the enamel plate at one end, and separated from it by a gradually increasing interval towards the other, the intervening stratum of milk is made to assume the form of a thin wedge. If the fluid under examination be of a rich quality, abounding in oily and caseous particles, it will possess such an amount of opacity that only a few degrees can be discovered on the subjacent enamel when the instrument is held opposite to the light; if, on the contrary, the specimen be of inferior quality, whether from innate poverty, or the admixture of water, the diminution of opacity thence resulting will be evinced by the enamel scale becoming visible through a deeper part of the fluid, or at a greater distance from the commencement of the scale; the degree of translucency, therefore, can be measured by the number of lines visible through the fluid." [The following simple test by which to judge of the qualities of *human milk*, given by COLOMBAT, is less extensively known than it deserves. He directs a drop to be placed on the finger-nail: if it adheres to it at first, and then spreads, without running, it is in the natural condition; in the contrary case it is not sufficiently consistent; while it is too thick if the drop adheres to the nail without spreading.—(Colombat de l'Isère, translated by Meigs, Phil., 1845.]

2. Dr. Huppert presents an elaborate report of the labors of E. H. v. Baumhauer, (Versl. en Mededeel. der K. Acad. van Wetensch. Afd. Natuurk., p. 145–188, 1858,) and C. Trommer, (Die Prüfung der Kuhmilch, 8, pp. 37. Berlin: G. Bosselmann, 1859.) The different modes of detecting adulterations, and testing for and estimating the various articles intermixed, are very clearly presented and practically illustrated. To enter into details would lead us beyond the limits of this *Summary*.

3. Dr. Seller offers some interesting observations on each of the two methods of estimating the quantity of solids in the urine, viz.: 1, by evaporation to dryness; and 2, by reference to the density. We quote:

"I lately extemporized an urinometer, on the plan of which a convenient instrument might, I think, be made. I took a small pipette, and having closed the lower aperture by the gas flame, I put a sufficient quantity of mercury into the interior to cause the instrument to sink perpendicularly below the level of the bulb. The pipette now floated, in pure water, perpendicularly with the water-line, nearly midway between the bulb and the upper extremity. I then fixed a thin slice of a common cork on the upper end, so as to form a small table on which weights might be laid. I also passed a wire with a slender point perpendicularly through the cork till it touched the surface of the water, to serve for an exact index to the height at which the instrument stood in the water. When this rude instrument was placed in a fluid denser than water, it rose, of course, throwing even a portion of the bulb above water; but, by placing weights on the little cork table, it was made to sink to the level at which the point of the wire grazed the surface of the water. Since by this means the same bulk of fluid is evidently always displaced, whatever be the density and whatever the temperature at the time, provided the weights on the little cork table are adjusted to keeping the point of the perpendicular wire just grazing the surface of the fluid, those weights will exactly indicate the difference between a given bulk of distilled water and the same bulk of any denser fluid. Whatever be the temperature at the time, the index for distilled water is shifted to answer for that temperature. If the bulk of fluid displaced by such an apparatus be not known, all that is requisite, for example, in the case of the urine, is to immerse the instrument in a fluid known to have a density of 1060, and to ascertain how many grains suffice to sink the point of the wire to the surface; and the product of this number divided by 60 will answer to the weight required for each degree between 1000 and 1060. Here there is no inequality of degrees. The sixtieth part of the difference between the weight of the volume of distilled water displaced, and the same volume of the standard fluid at 1060 equally displaced, corresponds to a degree. I find it convenient to use pieces of thin lead-wire in such experiments, owing to the facility with which these can be adjusted to any weight. Such an instrument, of course, is better suited to the study than to the sick-chamber.

"With respect to the determination of the proportion of solid matter in the urine by evaporation to dryness, it has long been manifest that the difficulty of the ordinary methods place them entirely beyond the reach of medical men for practical use. A mode, however, occurred to me some time ago, which I have repeatedly put to trial. This mode is, I think, susceptible of very great accuracy, and at very little cost of time. It must be confessed, nevertheless, that without due precautions, it may lead into very considerable errors. It is founded on so simple an observation as that common filtering paper readily takes up a large proportion of urine, and, under favorable circumstances, dries in no long time. The only considerable source of error is the difficulty of bringing back the paper to exactly the same hygrometric state in which it was at the beginning of the experiment. The paper is to be carefully weighed before being moistened, and weighed again with the same care after it has become dry—the difference in weight is the amount of the solid matter in the quantity of urine employed. For 1000 grains of urine, or rather for 1000 water-grain measures of urine, I have commonly used three sheets of filtering paper, cut each into

four pieces. The urine is poured into a well-dried basin, and each piece is moistened in succession, as far as it will bear without dripping; enough being left unwetted at its upper part to allow of a fold, by which it may be hung on a string or wire to dry. For this effect a few hours suffice. If the temperature and hygrometric state of the apartment can be kept uniform during the whole process, there is plainly no difficulty; if the drying be carried too far, there is an error of deficiency in the amount of the solids inferred; if the drying be insufficient, or if the apartment has declined in temperature, or its hygrometric state has become greater, there is an error of excess. It should be remembered that the paper, though apparently dry, readily absorbs moisture, owing not only to its original tendency, but to the deliquescent character of the solid matters contained in the urine."

4. Dr. McGhie has entirely modified Pettenkoffer's and Runge's tests. He says, "When concentrated sulphuric acid is added to an equal volume of diabetic urine, and heat applied to ebullition of the mixture, the following changes occur: First, the fluid becomes blackish, and soon passes into a state of inky blackness. As soon as ebullition commences, the mixture begins to froth up, and continues to be covered with a layer of froth so long as the boiling continues. This layer of froth also remains after the mixture is cooled. In no other kind of urine except the diabetic are these appearances manifested.

"After the boiling has been continued for a short time, besides the intense black color, when the fluid is closely examined there will be found a copious evolution of carbonaceous particles. The high specific gravity of the fluid in which these fine black particles are suspended, prevents their being deposited as a precipitate till it is largely diluted with water. In ordinary samples of diabetic urine a copious precipitate is speedily deposited on the addition of water. The precipitate should now be thoroughly washed on a weighed filter, till all the acid is removed, and nothing but the pure carbon remains on the filter. After being carefully dried and weighed, the excess of weight of the filter containing the carbon over the weight of the filter without the carbon, will give the amount of carbon contained in the sugar; and when this quantity is found it is easy to calculate the weight of the sugar. The dried precipitate on the filter will be found in the form of numerous small pieces of jet-black carbon, which may be collected, and, by combustion, will be dissipated in the form of gas, without leaving any solid residue."

5. *Etoc Demazy* having asserted that the anatomical cause of stupidity in the insane was œdema of the brain, Marcé determined "with all precautions" the amount of water at 100° C., and found:

	Man, (average.)	Lamb.	Calf.	Ox.	Rabbit.	Pheasant.	Owl.
In 100 parts gray substance,	80 parts water.	83.6	85.6	82.6	79.2	82.7	76.2
" " white "	70 "	70	69.8	63.7	64.3	76.9	66.7

According to this result the gray substance of the brain in man, as well as the lower animals, contains more water than the white.

He also examined whether the brain is able to take up more water than it already contains. He injected pure water into healthy brains, and determined the amount; then he macerated weighed pieces of brain for 24, 48 hours, and longer, in water, and weighed them again. He found that the pieces had absorbed 50 per cent. of their weight of water; *i. e.*, that 30 parts of solid sub-

stance corresponded no longer to 70 parts of water, but to 150. In confirmation, he found that brains with membranes infiltrated with serum contained more water, especially the gray substance, (f. i. 85 and 90 per cent.) than normal brains.

6. On opening a tightly fitting tin box, in which a quantity of salmon-roe paste had been kept for nearly three years, the organic matter was found covered by an efflorescence of acicular crystals, which consisted entirely of chloride of sodium. Solution and spontaneous evaporation caused the deposition in the ordinary or cubical form.

"The crystals, some of which are nearly half an inch long, appear to be rectangular prisms terminated by four-sided pyramids. They are beautifully clear, colorless, transparent, elastic, longitudinally and transversely striated, and many are bent or contorted in a manner similar to the native hydrated sulphate of lime, called selenite by mineralogists." "It is singular to remark that, at all events, as far as we know at present, the acicular varieties of the chlorides of potassium and of sodium are only developed in the presence of organic matter, just as the production of octahedral chloride of sodium appears to be due to the solution from which it crystallizes, containing urea. Since writing the foregoing I have observed an efflorescence of acicular chloride of sodium on an animal deposit which was sent me for analysis, and which had been originally mixed with a solution of common salt to prevent it undergoing putrefaction."

7. "When the bark of the Linden is boiled for some time in water it becomes soft, supple, and susceptible of taking all kinds of forms, which it preserves on becoming hard by cooling. This property it preserves after having been used, so that it can be used again for different purposes." If this proves true, the bark of the linden may be, to a certain extent, substituted for gutta percha.

II. ANATOMY AND PHYSIOLOGY.

9. *On the Normal Structure of the Liver.* By Prof. E. Wagner. (Archiv der Heilkunde, I., 3, 1860, p. 251–272.)

10. *The Vena Ophthalmo-meningea.* By Prof. Jos. Hyrtl. (Oesterreich. Zeitschrift für prakt. Heilkunde, V., 49, 1859.)

11. *On the Arteria Profunda Femoris.* By Dr. Johann Srb. Oesterr. Zeitsch. für prakt. Heilk., VI., 1860.)

12. *The Preservation of Corpses with Acetate of Alumina.* By Prof. Burow. (Deutsche Klinik, 8, 1860.)

13. *On Muscular Contractions from Mechanical Irritation in the Living Man.* By Dr. L. Auerbach. (Froriep's Notizen, III., No. 8, 1860.)

14. *Experimental Researches on Regeneration of Nerves separated from Nervous Centres.* By Drs. J. M. Philipeaux and A. Vulpian. (Gazette Médicale de Paris, August 11, 1860.)

15. *The Influence of the Curare Poison upon Nerves and Muscles.* By Drs. Wundt and Schelske. (Schmidt's Jahrbücher, CVII., No. 7, 1860.)

16. *On the Functions of the Cœliac and the Mesenteric Plexus.* By Prof. Budge. (Nova Acta Acad. cæs. Leop.-Car. Nat. Cur., XIX., p. 257–284, 1860.)

17. *On the Pancreatic Secretion.* By M. Claude Bernard. (Medical Times and Gazette, August 25, 1860.)

9. The most important point in this real contribution to the knowledge of the intimate structure of the liver, is the demonstration that the hepatic cells are separated from the blood by *two* membranes, the capillary and a tunica propria. The former is easily recognized by the well-known oval nuclei, and the latter, the "basement membrane," (with the proof of the existence of which the author settles a much contested question,) is also distinguishable, according to him, by peculiar nuclei in its cells. They are best seen in infantile livers. They are mostly round, average $\frac{1}{400}$ of a line in diameter, have a sharp outline, have clear, slightly granular contents, and generally a distinct nucleolus.

10. The ophthalmo-meningeal vein has never before been described. On raising the anterior lobe of the brain, it may be seen on the side of the sella turcica in every case, without exception, according to the author. It is either single or double, extends from the fossa of Sylvius to the superior orbital fissure, and is generally of the thickness of a knitting-needle, varying from that to the size of a quill. On tracing it towards the orbit, it is found in most cases to empty into the spheno-parietal sinus on the under surface of the lesser wing of the sphenoid; in other cases, it passes the sinus and unites with the lachrymal vein; more seldom with the ophthalmo-facial that passes through the inferior orbital fissure; and more seldom still, crossing the rectus superior muscle, it unites with the vena ophth. cerebralis. In one exceptional case, it gathered the blood of the posterior ciliary veins.

In spontaneous or traumatic inflammation of the orbit and its contents, phlebitis may extend through this vein, directly to the pia mater.

11. Dr. Srb examined 100 bodies or 200 extremities with regard to the anatomical relations of the profunda femoris, *i. e.*, the "muscularis profunda" branch of the crural artery. Its chief branches are always laid down as, 1st, the *circumflexa fem. externa*, to the muscles on the anterior and external aspect of the thigh, with a *ram. descen.* to the extensores cruris; 2d, the *circumflexa fem. interna*, to the inner muscles; and 3d, the three *perforantes*, to the inner and posterior side of the thigh; and the author found this arrangement in 124 cases out of the 200 to exist: viz., In 42 bodies on both sides, 20 times on the right side, and 20 times on the left.

Independent origin of the circumflexa interna from the crural artery (so that the profunda fem. comprises only the circumflex externa and the perforantes) was found 41 times, viz., 7 times on both extremities, 14 times on the right, and 13 times on the left.

Independent origin of the circumflexa externa from the crural artery (reducing the profunda to the circumflexa interna and perforantes) was met with in 26 extremities, 3 times on both sides, and 10 times each on one side.

Independent origin of the perforantes by a common stem, and consequently reduction of the profunda to the two circumflexa, was found twice on the left side.

Independent origin of all three divisions of the profunda was found 7 times, viz., in a female subject, on both extremities; and three times on the right, and twice on the left side in males.

12. The solution of acetate of alumina (obtained either in the usual way of adding to a solution of alum one of acetate of lead, or according to the author better, by rubbing together 5 ounces of alum and 6$\frac{3}{4}$ ounces of acetate of lead,

and then adding 32 ounces by weight of water,) is injected into the crural artery, until the vessels will take up no more, then into the trachea until the liquid flows in large quantity from the mouth, and finally into the bladder and the rectum until no more can be got in. The author relates some remarkable cases of preservation of bodies thus injected, and dried.

13. Dr. Auerbach demonstrated to the Medical Section of the Schlesische vaterländische Gesellschaft, at its session of 1860, three series of phenomena observed by him on striking muscles with a percussion hammer or the finger. The first embraces the instantaneous contraction of the whole muscle when its whole breadth is touched, or of the particular bundle or bundles of fibres struck, when the blow is longitudinally limited. The blow need be but slight, and its effect is apparent to the careful observer, both by sight and on the touch. This, Dr. A. regards, not the result of reflex action, nor of immediate muscular irritability, but as a *neuro-muscular* contraction. The second series is one of *idio-muscular* contraction. It can be distinctly seen and felt as a limited hardness or tumor arising from the contraction of the fibres touched, and generally having the form of the instrument used, viz., while the blunt edge of a knife causes a straight-lined rising or swelling, the touch with a ring causes a ring-form hardness, the square base of a hammer, a similarly-formed swelling, &c. The third result, observed from a blow, is a kind of *peristaltic* motion of the obliquely striped muscular fibres, giving the appearance of a wave rolling along the fibres of the muscle, from one end to the other and back again. Dr. A. demonstrated these interesting facts before the Section, especially on the biceps muscle of a blacksmith, who had a very muscular arm, and being affected with tuberculosis, was greatly reduced in fat. [Among the practical applications made of these observations, we only refer to the explanation of the benefits resulting from systematic blows on muscles as a means of inciting them to action, as employed in some species of medical gymnastics.]

14. The account of the researches not being entirely published, we shall most likely have occasion to refer to them again. As yet, their principal result seems to be the determination of the important fact that the alteration which the nervous tubes undergo when nerves are cut or resected, is limited mostly to the disappearance of the medullary substance, and that the re-establishment of the normal condition consists principally in reappearance of this substance. From this it follows that the altered condition is not a destruction, but a modification of these tubes, and that their return to the healthy physiological state is not a regeneration in the proper sense of the word, but simply a restoration.

15. I.—The state of the nerves after curare poisoning is not identical with death; excitability may, therefore, after lapse of more or less time, return, after even the most intense poisoning. II. In all cases of curare poisoning there is a stage in which reflex excitability is increased. III. The nerves of poisoned parts, in partial poisoning, are always able to continue reflex impressions for a long time after the cessation of their direct excitability. IV. The number of heart beats is increased after the poisoning. The action of the Vagus does not cease after the poisoning, but it shows an activity reverse to the normal; the nerve is tetanized, the cardial pulsations being rendered more frequent.

From the highly interesting facts established by the authors, it is evident that it has been an error to regard nerve-activity after curare poisoning completely abolished even in the terminal branches of the nerve; and that, therefore, the

experiments with curare cannot be brought to bear on the question of "irritability."

16. Numerous experiments of extirpation of the cœliac ganglia and the mesenteric ganglion were performed on rabbits. The animals generally died within twenty-four hours after the operation; in a few cases death did not occur till the third day. The effects of the operation, briefly reviewed, are—1, Softening of the fæces, so that frequently no consistent mass is found in the rectum, but only a completely soft mash; 2, increased motion of the bowel; 3, elimination of tough, glairy mucus and blood; 4, enlargement of the liver; 5, manifestation of pain and suffering, unless the splanchnic nerve had been previously cut.

17. After speaking of the various methods of collecting the produce of glands connected with the digestive apparatus and the modifications required in each case, Bernard refers to the fact that the chemical composition of the saliva is entirely independent of the general state of the animal's health, while the reverse is true of the pancreatic secretion; hence, when peritonitis, which so often attends operations on the abdominal viscera, occurs, it is impossible to obtain healthy pancreatic fluid; and hence the absolute necessity of establishing definite rules for the performance of these operations. Experiments on the pancreatic secretion must be performed while the digestive process is in full activity; and Bernard prefers the method adopted by Tiedemann and Gmelin, in operating on the dog, to all others.

EDITORIAL AND MISCELLANEOUS.

— The season has once more arrived for the opening of our medical schools. In this city, unusual preparations have been made, and all the Colleges are prepared to begin the Session of 1860–61 with an ardor never before witnessed. The preliminary courses which have been instituted in all, and which have finally become a necessary part of the academic instruction of the student, have fairly commenced. These lectures—introductory to the regular course, and extending through a month—are not exclusively given by members of the Faculty, but in many instances by aspirants to professional dignities, for the enunciation of new views or new thoughts, as well as by those whose delegated province it is to teach. They often are, then, more interesting and quite as instructive as those of the regular course. The special subjects taken up are generally such as the lecturers most delight in, and upon which they have observed much and studied much; consequently they enter upon them with pleasure, lecture with more verve, exhibit more originality, abandon themselves to their own personalities, and by their enthusiasm attract the student. This is the

secret of the successful lecturer, and for these reasons, the preliminary courses given, as they frequently are by the professors and the attachés of the school, upon favorite subjects, become quite as important to the student as the regular course, and we trust that they will continue to grow in favor, and that each school will attract to itself those young men whose ardor leads them to investigate for themselves any special subject, and that the students will show their estimate of this plan, by a rigid attendance upon them.

To show that our view of the subject has been anticipated by actual illustration, we have but to instance the subjects of the preliminary courses now given in this city.

At the New York Medical College special lectures are given on Hygiene and Prophylactic Medicine; Visceral Anatomy; Diseases of the Breast; Amputations; Dentition; Endosmose and Exosmose; Venereal Diseases and Operative Midwifery. At the College of Physicians and Surgeons, on Meteorology; Anatomy of the Brain; Organs of Special Sense and Venereal Diseases. And at the University Medical College, on Physiology; Practical Medicine; Poisons; Microscopic Anatomy and Venereal Diseases, all more or less illustrated at the various clinics which occur almost daily.

This enumeration shows a range of subjects which cannot fail to be attractive to the student.

The only change made in the Faculty of the College of Physicians and Surgeons is the resignation of Dr. George T. Elliott, of his Adjunct Professorship. The Faculty of the University remains unchanged.

At the New York Medical College there has been an entire reorganization; but two of the former Faculty remaining, while additional chairs have been created, so that now the Faculty numbers twelve.

An important step has been taken by this school, which, we hope, will speedily find imitators. A Hospital has been founded, and a portion of the College building set apart for this purpose. It is already in working order. Upon a recent visit to it, we were struck with the admirable arrangement of the larger room, devoted to this purpose, and would venture to predict a successful result to the project. The patrons of this new charity have happily called it "The Charity Hospital;" and we hope it will become the nucleus of an extensive institution, where clinical teaching in connection with this College will be one of its principal merits, as it now is one of its aims.

—The American Pharmaceutical Association held its Ninth Annual Session in this city, commencing September 11, and continuing four days. Many reports were read, which we shall notice when they appear in the published Transactions of the Association. A lengthy discussion took place upon the merits of the new Law for regulating the Sale of Poisons. The conclusion arrived at, that though faulty in many of its provisions, it was yet a step in the right direction, and many valuable suggestions were made by those who took part in the discussion, for remedying the objectionable portions of the law. A committee of three, to "mature a plan by which the objects (of the law) may appear to be best attainable," was appointed.

From this committee much is to be expected, for certainly they who are familiar with the merits of the subject, and for the guidance of whom the law was instituted, are best calculated to discover its shortcomings, to suggest the remedy or to provide a better substitute. The report of this committee will, therefore, be looked for with much anxiety by those who are impressed with the importance of this subject.

The Pharmaceutists of New York, at the close of the session, welcomed the members of the Association from other cities by a complimentary dinner, to which were invited several of the medical profession, including the President of the Academy, the President of the College of Physicians and Surgeons, and the members of the medical press, and several of the Professors of the Medical Colleges of this city.

Among the speakers who responded to the regular toasts of the evening were Dr. John Watson, Dr. Reese, Dr. Doremus, Dr. Gardner, Dr. Griscom. The occasion was one of much pleasure to all the guests.

— The length of the articles in this number has necessarily obliged us to defer to the next number several Reviews and Book Notices which we had expected to have included in our pages this month. Publishers must have patience with us. For the same reason that these papers have been deferred, we have curtailed both the *Summaries* which were prepared for this number. These will also be included in future numbers.

—A new journal, to be exclusively devoted to the subject of Materia Medica, is announced. It is to be called the "American Journal of Indigenous Materia Medica and Repertory of Medical Science." It is to be published monthly, by Messrs. B. Keith & Co. The first number will appear in November.